STRICTURES

ON THE

ELEMENTA MEDICINÆ

OF

DOCTOR BROWN.

BY

PHINEHAS HEDGES.

HUMANUM EST ERRARE.

GOSHEN:

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MDCCXCV.

To the PHYSICIANS of the UNITED STATES.

Gentlemen,

THE following observations were committed to paper in the intervals of business; perhaps they want that connection necessary to give a clear and comprehensive view of a subject involved in so much obscurity by the author of the *Elementa Medicinæ.*

The necessity and propriety of animadverting upon *Doctor Brown's* theory, I have long thought sufficiently obvious; and I have waited with a considerable degree of impatience in expectation of some more able pen to take up the subject—But as no combatant has appeared, I think myself amply justified in entering the field of argument.

I have all along adhered to the new phraseology of the author; not so much on account of its aptness to express the different conditions of the animal œconomy, as to shew the inconsistencies and futility of his principle.

In my quotations from the ELEMENTA, I have in many instances abridged the verbosity (or rather expletiveness) of his style; but in no instance have I intentionally altered the sense of the paragraph. If it is necessary that I should declare and avow the motives which led to these strictures—I answer, that a forcible conviction of the dangerous tendency of extending the stimulant

mulant plan of cure, introduced and impreſſed with ſo much violence by the author, was the moſt urgent inducement—And whatever reception it meets with, I am conſcious that I have uniformly endeavored to ſet forth the truth.

The number of *Doctor Brown's* diſciples in this country, is to me unknown. But whether ſmall or great, it would not be unexpected for my work to meet with many a ſevere criticiſm in their hands. I am perfectly willing it ſhould be brought to the touchſtone of reaſon and diſcuſſion; and if any one diſcovers any material error, I ſhall acknowledge it.

THE AUTHOR.

Little-Britain, Ulſter County,
Auguſt 10th, 1795.

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THE world has lately been favored with a production in the medical art, which if its merits were equal to the boaſtings of its author, we would not have occaſion to lament the fatal termination of a great part of the diſorders which afflict the human race. But unfortunately, this boaſted production, when examined, and its principles applied to practice, leaves the profeſſors of medicine in the ſame uncertainty and conjecture, which has ever pervaded that ſcience. The author in his preface, begins with an affected and falſe repreſentation of the dark, ignorant, and baneful ſtate of medicine when guided by the theories heretofore and at preſent in vogue among the profeſſors of medicine. He repreſents himſelf to have been for a number of years in a wilderneſs of darkneſs;

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to have followed with implicit faith the wild conceits of former theoriſts—But to his utter aſtoniſhment, out of this great and impenetrable darkneſs, he was brought into marvellous light. It was at that important æra that the genuine principles of medicine were diſcovered; for what was called medicine before, was a falſe conception, and always in practice ended in abortion.

This diſcovery was not the reſult of reaſoning from analogy; for every principle heretofore ſet up to unravel the myſteries of the animal œconomy were illuſory and equivocal. The author undoubtedly had a deſign in this repreſentation of his diſcovery. Unwilling that the world ſhould believe his ſyſtem, reſembled the ſyſtem of Cullen—he took uncommon hypocritical pains to amuſe the public with the pretended novelty of its principles. It requires but a ſuperficial acquaintance with the works of Cullen, to diſcover that the principle laid hold of by Doctor Brown, and carried through his work is nothing, more or leſs, than the principle of Cullen, uſed in explaining the different conditions of the brain in *mania* and *melancholia*, and that principle diſtorted and mutilated by his transforming power, into what he confidently aſſerts to be a new principle in medicine, and ſufficient to explain all the phænomena of health and diſeaſe. By this diſcovery, he has changed phyſical incertainties into mathematical certainties.

He has pointed out the ſalutary and hurtful powers

powers of the human body; he has invented a ſcale, a criterion by which you can diſcover the former and avoid the latter, and by which you can always aſcertain whether the excitement is raiſed or fallen below the point of health—In ſhort, by this effulgent diſcovery, medicine is reduced to ſuch a degree of ſimplicity, that it requires very little knowledge except a ready acquaintance with addition and ſubtraction. For if the excitement is two degrees above the healthy point, ſubtract two; and if two below, add two, and health will infallibly ſucceed. Having made theſe general obſervations on the *Elementa Medicinæ*, we propoſe to make ſome ſtrictures on the work, in the courſe purſued by the author. In the courſe of theſe obſervations I may occaſionally touch upon the theory of Doctor Cullen, in order more forcibly to illuſtrate my ideas—and if it appears that I am a diſciple of Cullen, it is from a thorough conviction of the truth and importance of his doctrine. I by no means wiſh to be a blind ſupporter of the writings of Cullen, but I think their truth, ingenuity and excellence, are ſufficiently obvious.

In the beginning of his work our author defines medicine to be preſerving the good, and curing the bad health of animals. Is this definition logical? Is not health a point the leaſt deviation from which conſtitutes diſeaſe? I would not wiſh to appear captious; but an author who challenges all the world, ſhould be ſeverely criticiſed.

Paragraph VI. *General diſorders are always, and local never preceded by prediſpoſition.* I object to the firſt part of this propoſition, for this reaſon, that perſons who from all appearance of ſenſe, life and motion, are endowed with the ſame temperament of body, are not all affected with diſorders which are ſuppoſed to be epidemic in epidemic ſeaſons; and further, that perſons whoſe temperament appears moſt repugnant to the operation of cauſes which produce epidemics are ſometimes affected as well as thoſe of a temperament more favorable to the operation of epidemic cauſes, and that in the moſt violent manner. But if prediſpoſition always preceded general diſorders, we ſhould ſuppoſe that prediſpoſition together with the operation of general cauſes affecting all men alike would uniformly in perſons prediſpoſed produce the diſorder. But this is contrary to experience, and medical facts are ſo various and diverſified that it is difficult to eſtabiſh this opinion on any ſure foundation. Local diſorders, as boils and eruptions; as certainly depend on the diatheſis as general diſorders and according to our author the diatheſis is both prediſpoſition and the diſorder. It is a very rare occurrence for boils, which are the effect of an inflammatory diſpoſition, to affect perſons of an emaciated ſpare habit of body. And as they are the conſtant attendants (whenever they happen) of an inflammatory diatheſis, we ſuppoſe they are accompanied with pre-diſpoſition the diatheſis.

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Our author in paragraph LXXIII, defines prediſpoſition a middle ſtate between health and diſeaſe; but this definition conveys no adequate idea; for prior to our conceiving what he means by the term pre-diſpoſition, we muſt underſtand what is meant by health and diſeaſe. He has confined himſelf to the literal etymology of the word which ſignifies to be placed, or go before a thing which ſucceeds it; but this definition conveys no idea of the ſtate of the ſyſtem. In a ſucceeding paragraph, he ſays pre-diſpoſition ariſes from the ſame exciting cauſes, acting upon the ſame excitability, and in fact is part of the diſeaſe. Prediſpoſition I conceive to be a peculiar diſpoſition or organization of the muſcular fibres of the body, either laid in the original ſtamina, or induced by diſeaſe or habit. The muſcular fibres of the living ſyſtem, are more or leſs tenſe and elaſtic, and are deſtined to act with more or leſs vigor—but as there is a certain point of tenſeneſs and elaſticity of muſcular fibre common to the whole, or almoſt all the human race, and as yet there may be conſiderable variations in different perſons, without impairing health, phyſicians have denominated this deviation from the common ſtandard, pre-diſpoſition. It will readily be ſeen that pre-diſpoſition may be coeval with the firſt rudiments of the fœtus. It is very apparent that ſome children have more lax and flaccid muſcular fibres than others; whether owing to the rudiments of the fœtus or circumſtances affecting its nouriſhment in utero, we will not affirm. A perſon of a lax fibre, is generally

nerally supposed to be more disposed to disorders originating from debility, than a person of a tense elastic fibre. The lax fibre however, is constitutional and not owing to the same exciting powers which produced the disorder and constitutes no part of it. The same quantity or energy of exciting power that will produce a disorder in one person, will not in another, which is the reason why we say the former is pre-disposed to the disorder, and the latter not. Our idea of pre-disposition is synonymous with our author's, more or less abundant excitability of the animal œconomy—He says that there is a certain portion of excitability assigned to every living system, and it will readily be seen that where there is a great portion of it assigned, that body will be affected both with the salutary and hurtful powers more than a person possessing less of it;—from whence it clearly follows that a person possessing a great degree of excitability is pre-disposed to disorder of the inflammatory class, and a person possessing less, to disorders originating from direct debility.

Paragraph XXI. Our author has introduced a new theory of the passions. In the explanation of this subject, he affirms that the sedative passions as fear and grief arise from the same causes, namely exciting powers—Although grief and fear are produced by powers which may be called stimulant, or exciting in comparison of the entire absence of those powers, yet in comparison of the natural excitement of the mind they pro-

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duce a ſedative effect, and may therefore according to the author himſelf be ſaid to be produced by ſedative cauſes. For in a note upon paragraph XX. he introduces what he calls a new mode of reaſoning into medicine, that is, from an identity of effect, he infers an identity of cauſe. In the paſſions of grief and joy, two effects diametrically oppoſite, both phyſical and moral are produced; from whence we infer a diverſity inſtead of identity of cauſe. Our mode of reaſoning on this ſubject may aptly enough be compared to the different national denominations of money—here we eſtabliſh an unit, and our calculations always have a direct reference and compariſon with this eſtabliſhed unit. The natural excitement of the mind may be called the unit, from which we reckon the various degrees of health and diſeaſe, and the effect produced on one ſide of this unit we attribute to the operation of different cauſes from that on the other. A paſſion which lowers the excitement and vivacity of the mind, we call a ſedative paſſion, and that for the ſame reaſon that we ſay a ſhilling is leſs than a dollar, and more than a penny. The author's reaſoning amounts to as great an abſurdity as to ſay that as all the cauſes which beget and ſupport vegetable life are ſtimulant, therefore all the variety of vegetables are one and the ſame production. The diſcrimination of viſible and mental objects is of the utmoſt conſequence in the communication of knowledge.

In a note upon paragraph XLVII, we are told

told that indirect debility commences at 70° of his scale and continues to 80°, the death point. From what data he infers that indirect debility, begins at the precise point of 70° he has not informed us, neither does it appear to me that this boasted scale by which we are to guage the vital principle, can be of any utility or advantage in the administration of medicine—some of the disorders placed above 70°, appear to me to depend on too much phlogistic diathesis, or excitement; whereas if they were the effect of indirect debility a mode of cure opposite to that generally followed, should be adopted in those disorders. It would appear rational to conclude, that whenever indirect debility begins, that the excitement should be lessened. Our author however, supposes that the excitement is still increasing from 70° to 80°, notwithstanding the approach and existence of indirect debility, which is as absurd as to affirm that 70° minus 10 amounts to 80°. The continued repetition and operation of exciting powers, often induce indirect debility, when their aggregate operation has never at any one time been sufficient to raise the excitement to 70°. In the apoplexy we generally suppose a rigid tense fibre to exist, and the symptoms all serve to evince that this disorder depends on too great an excitement or inflammatory diathesis, and the mode of cure in these cases is conformable to this generally received opinion. We imagine the author to have denominated an effect of disorders, their cause. The idea of indirect debility pre-supposes that the exciting powers

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have been applied to a very considerable degree, and as whenever the excitement is raised above 40° and keeps within the range of 70°, a disorder depending on too great excitement must exist in some of the intermediate points, it clearly and irresistibly follows, that a sthenic disorder must precede an anti-sthenic one depending on indirect debility. Disorders of indirect debility are most generally the consequences of continued and habitual courses of gluttony and ebriety.

The operation of contagion and marsh effluvia, I presume to be of a sedative nature, or they possess a power of stimulating less than is necessary to support the healthy state of excitement. A person confined in a jail or prison-ship, never experiences increasing vigor prior to an attack of a jail fever. Whereas if the matter of contagion was of a stimulating nature a gradual increase of vigor would announce their stimulant operation. I will not contend with Doctor Brown, that these noxious agents stimulate none at all; it is sufficient reason for calling them sedative if they do not stimulate in a degree necessary to maintain the healthy state of excitement. It greatly strengthens this opinion that contagious matter generally exists in warm climates, warm seasons and confined places, when the elasticity and density of the air is greatly diminished.

From facts so conformable to experience we conclude, that diseases of indirect debility are not so numerous as Doctor Brown imagines, and that

that they may happen when the exciting powers have not raiſed the excitement to 70°, and that from their habitual repetition. It may be proper in this place to enquire what uſeful purpoſe the ſcale of our author can ſerve in the ſcience of phyſic. It may aſſiſt in explaining and illuſtrating the principles of his theory; but as its graduation is arbitrary, any other would have anſwered his purpoſe equally as well. Does it poſſeſs advantages ſuperior to the arrangement of noſologiſts, into claſſes which comprehend a very general aſſociation of diſeaſes; into orders which are a ſubdiviſion of claſſes, and into genera and ſpecies which are more minute ſub-diviſions. In the firſt, a very general character of diſorders is given; a few of their ſymptoms which are common to the whole of the claſs are noticed; in the orders a more particular character is given, and in the genera and ſpecies a very minute detail and deſcription of all the ſymptoms. By this arrangement, the ſtudent's attention is confined to one preciſe object. But in the general abſtract manner in which the author has treated this ſubject, a ſtudent could never acquire preciſe ideas of the variety of complaints to which the human race are liable. A competent, adequate knowledge of the vegetable and animal kingdoms could not be obtained unleſs botaniſts and natural hiſtorians gave a minute deſcription of any production under inveſtigation, ſhewing wherein it differed from others of the vegetable tribe—Noſologiſts may have made more diſtinctions than have any

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real or useful foundation, but our author in his furious rage for novelty has levelled the arrangements of ages. I submit it to the candor and good sense of the faculty to declare whether this innovation is useful or just—diseases in their different stages are attended with different degrees of excitement, the principle of life is continually wavering, and he that is the least experienced must have observed inflammatory disorders exhibit strong marks of debility.

Paragraph XLVIII. *The seat of excitability is medullary, nervous matter and muscular solid.* But how confined or universally diffused this medullary matter is, or in what manner excitability, or excitement, is produced, the author is entirely silent. It is the professed object and design of theory to explain in a rational, consistent manner every phænomena of the animal œconomy, both in the healthy and diseased state; and we judge of the truth and utility of theory from its perfectly answering this end. It is a desideratum not yet ascertained. The essay of Doctor Brown has not enlarged the sphere of theoretical principles. Theorists have heretofore examined the fluids and solids in search of the true principles of medical science: they have by dint of experiment, discovered some of the properties both of solids & fluids, and as far as those discoveries went, applied them to the elucidation of the phænomena of the living system. But Doct. Brown, instead of exploring the animal œconomy, has set up an abstract principle.

He has neglected experiment, the clue of modern philoſophy; he has incarcerated his ſenſes and cloſed up every avenue of knowledge. The term excitability, conveys no definite idea of the ſtate of the ſyſtem either in a healthy or diſeaſed ſtate. He does not explain the ſtate of the ſolids and fluids in health, nor the change they undergo in diſeaſe. Where is the ſeat of medullary nervous matter? Is it in the adipoſe membrane? Is it univerſally diffuſed over every fibre —and is it generated in the brain—and what is the proceſs by which food is converted into this vital ſubſtance? From what appearances do we judge that medullary nervous matter is abundant or deficient, or in proper degree? Is excitability inherent in medullary matter? Does it depend on a particular contexture of the primordial particles of medullary matter? Or does excitability, like the electric fluid, remain in a dormant ſtate until the exciting powers, like excitation, arouſe it into action?

Anſwers to theſe queries are eſſentially neceſſary to the illuſtration and completion of his theory. It being the foundation of his ſyſtem, together with the hitherto inexplicable nature of the ſubject, I ſhould have expected would induced the author to have treated the ſubject in a more diffuſive and ſcientific manner. We preſume that the notion of medullary matter's being the ſeat of excitability, is not a part of that gleam of light that beamed upon his mind at the time of his diſcovery, but conclude the doctrine

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to have been taken from Kirkland and others who slightly touched upon this subject. Excitability in its seat, causes and effects, are but indifferently explained; and, in fact, the author is so candid in the beginning of his work, as to say the uncertain nature of the subject and the poverty of language rendered it difficult to speak very intelligibly on the subject. I ardently wish the author had pointed out the laws which regulate the excitability and excitement in a more perspicuous manner. But we are informed of this *only*, that in proportion as the excitement increases, the excitability decreases. This was known to every one the least acquainted with the animal œconomy before the luminous discovery of Doctor Brown: For as there is a certain degree of activity or energy capable of being exerted, when that energy is raised, there must be less to raise, unless the quantity was infinite.

Paragraph L. The author endeavors to inspire a belief that the notion of medicines operating on a particular part, in preference to every other part of the system, has been exploded in consequence of his discovery. It is well known, however, that it has been an obsolete, exploded opinion for near half a century. But the author must represent the system of medicine different from what it really is, before he can convince the world that he has effected a revolution in the principle of medicine.

Paragraph LXIII. *Medicines are not to be directed*

reflected to the ſolids or fluids, but to the diminution or encreaſe of excitement.

I confeſs this to be a new doctrine in medicine, and I believe as falſe or imaginary as new. It is beyond all conception to conceive how a medicine can effect any change in the ſtate of excitement, without operating on the ſolid parts, the ſeat of excitement. The opinion of medicines operating on the fluids, has been ſatisfactorily refuted by Doctor Cullen. And our author, with an inſatiate deſire of novelty, has excluded both ſolids and fluids from his pathology of diſorders. In what part of the body does he expect medicines to operate? If they do not come into contact with ſome part we can expect no ſucceſs from their exhibition; and if they do come in contact with any ſentient part, an operation on the ſolids is the infallible conſequence. Medicines do not operate upon the living principle immediately, but mediately.

To direct medicine to the excitement only, appears like adminiſtering medicines to ſpiritual beings who poſſeſs neither parts nor ſpace, in conſequence of which the operation of medicines would be entirely incomprehenſible.

Although we generally ſuppoſe debility to be the effect of cauſes operating upon the vital principle, we commonly connect an idea of laxity in the ſimple ſolids, and our medicines are directed with a view of altering the contractility and tone of

of the fibres of the body. I would not be understood to mean the simple solids strictly so called, but every fibre, whether distinguished by the name of simple or living solid.

The force of cohesion in the component particles of a fibre, we suppose to be lessened in debility. This opinion our author confirms when he says that the force with which muscular fibres resist stretching, is their density. And as he affirms that the force with which a dead and living fibre resists stretching, is greatly different, and as every encrease of debility is an approach to the state of death, therefore the idea that the force of cohesion, or density in the component particles of a fibre is lessened in debility, is rendered probable from his own concessions.

There is a certain cohesion of every particle of the body on which the most perfect health depends, and on which healthy excitement depends; it therefore follows that the state of the solids is an object of no small consequence in the treatment of disorders.

To exhibit medicines with an abstract view of altering the excitement, is a scheme too visionary for a serious mind. Medicines act on the living system, not as matter simply, not as excitement simply, but on the two whose union we imperfectly comprehend. They act and re-act upon each other; they produce phænomena

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which the utmoſt induſtry and reſearch have never been able fully to inveſtigate and develope.

Paragraph LXV. *The notion of health and diſeaſe being different ſtates, is diſproved by the operation of the powers which produce them, and thoſe that remove them being one and the ſame.*

The powers which ſupport life and remove diſeaſe are the ſame, as they agree in the common notion of being matter, but in their effects on the diſeaſed and healthy ſtate, they are entirely and ſpecifically different. Food and medicine have a very different operation on the animal œconomy; and although the author may ſay their difference of operation conſiſts in degree only, there is this, however, to be obſerved, that the former is overcome and aſſimilated to the body, and the latter not. All farinaceous, ſaccharine and oily ſubſtances, are convertible by the powers of digeſtion, into ſuccum et ſanguinem; but a perſon may eat opium, mercury, and all the chemical medicines, and they will never be converted into thoſe juices which are ſuited to repair the daily waſte of the body. Medicines ſtimulate directly, and food indirectly. A full meal, when in contact with the ſtomach, will encreaſe the velocity of the pulſe in a ſmall degree; but food is not taken barely with an idea of ſtimulating the muſcular fibres of the ſtomach. Its ſtimulating and exciting the ſtomach into contractions promotes its digeſtion; but it ſupports life in a different manner. Withhold every nutritious

tritious ſubſtance, and exhibit a quantity of any ſtimulant power equal to a full meal, and life will be of ſhort duration.

The principal intention of food is to be converted into blood, and ſtimulate the ſanguiferous ſyſtem. Medicines operate on the nervous ſyſtem. Add to this, I aſſert the phyſical condition of the body in health and diſeaſe are entirely different. Are the freezing and boiling points of water the ſame? Does not health conſiſt in a proper degree of energy of all the functions? and does not diſeaſe conſiſt in a perturbed, diminiſhed or encreaſed energy of the functions? To render the abſurdity more palpable, —has he not defined pre-diſpoſition to be a middle ſtate between health and diſeaſe? Whereas if health and diſeaſe were the ſame, there could be no middle ſtate; for the very idea of a middle ſtate, ſuppoſes two extreme oppoſite ſtates.

Paragraph LXXXIX. *The ſame debilitating remedies that remove any one ſthenic diſeaſe, remove all of that claſs, and the ſame ſtimulant remedies that remove any one aſthenic diſeaſe remove all the reſt.*

To prove this aſſertion the author muſt either exclude a number of diſorders that he has arranged under the two different claſſes, or deny facts which every days experience inconteſtibly proves. The rickets, the ſcrophula, and many other diſeaſes which he arranges in his aſthenic form

form, too often elude the force and power of the moſt ſovereign tonic and ſtimulant remedies, while the intermittent fever, a diſorder of the ſame claſs, gives way with great eaſe to the efficacy of bark and wine. This fact is a full refutation of the aſſertion.

I believe that however greatly deſirable ſimplicity and uniformity may be in the cure of diſorders, we often fail by giving way to reaſonings of this kind. I acknowledge, however, that reaſoning from analogy is the only ſubſtitute for actual experience. But I wiſh to caution the faculty againſt an implicit belief and confidence in this fallible criterion of truth. The author, in his diviſion of diſorders into two claſſes, and his indications of cure in conſequence of the diviſion I fear is too general, and if indiſcriminately followed would be productive of ſerious and alarming conſequences to the unhappy ſufferers.

There are a variety of circumſtances to be conſidered in the cure of diſorders. A phyſician ſhould give a wider range to his contemplations than barely the ſtate of excitement. The ſtate of perſpiration, the alvine diſcharge, the quantity of urine, all ought to arreſt his attention. And although Doctor Brown may aſſert that a reſtoration of healthy excitement would eſtabliſh the natural vigor of theſe functions, I am diſpoſed to believe, that a medicine poſſeſſing a leſs degree of ſtimulant operation, and at the ſame time exciting perſpiration, would have a more

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ſalutary operation in many caſes than a medicine not diſpoſed to increaſe the cutaneous diſcharge, and yet poſſeſſing a greater ſtimulant power. The body was conſidered as a whole, long before the publication of Doctor Brown's theory; but that whole conſiſts of ſeparate parts, all connected and combined together by a general principle. The different parts being differently organiſed, and thoſe diſtinct organs having diſtinct offices aſſigned, will no doubt modify the operation of medicines, notwithſtanding their general operation on the excitability. It is an undeniable fact, that there are medicines which increaſe perſpiration and the urinary diſcharge, whoſe operation can in no manner be accounted for on the author's general principles. Would it not have been in more perfect conſiſtence with the author's theory, to have enjoined abſtinence from all remedies in ſthenic diatheſis. For as all the powers in nature are of a ſtimulant operation, although you adminiſter a medicine poſſeſſing 20° of ſtimulant operation in a diatheſis of 60°; yet as it ſtimulates ſome, it muſt ſerve to keep up the diatheſis longer. Phyſicians have generally adviſed abſtemiouſneſs in diet and plentiful dilution in inflammatory diſorders on this principle. In a ſyſtem which pretends to demonſtration and mathematical exactneſs and certainty, it would appear proper to ſtate the length of time neceſſary for a medicine to raiſe deficient, or diminiſh increaſed excitement to the point of health. This would have been the touch-ſtone of the theory under diſcuſſion. From

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the author's attention to exhibit a medicine of exactly as much less exciting power than 40°, as the excitement is raised above 40° it would appear that he is fearful of debility in the employment of less exciting power; this indirectly conveys an idea that the operation of medicines is instantaneous—for if their operation was not so, there would be no hazard in the administration of a medicine possessing less stimulant power than 20°, when the excitement is 60°. Experience however, proves that medicines are slow in their operation, and that in the morbid state a continual repetition of exciting power, is necessary to maintain the system even in statu quo. Let the excitement be reduced to 20° in a disease of debility, it would then appear according to the principles of our author that by adding to 40° (the stimulant power necessary to support health) 20°, that the excitement would be restored to the healthy point. But if we concenter all the stimulant and nutritious power of a full meal, and add half its quantity, the disease will maintain its obstinacy. The hurtful powers which produce disease, exist without the body, they operate on the surface, and medicine on the stomach; they are invisible, untagible and escape examination. It is not only necessary to exhibit a quantity of stimulus as large as is necessary to support health and take off deficient excitement; but it is also necessary to exhibit a power whether stimulant or sedative, sufficient to counteract those hurtful powers—for the cause is supposed still to operate during the exhibition

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of medicines, in consequence of which we have both cause and effect to combat. If the cause was removed, the power of nature would be sufficient to restore the healthy state. These observations apply to both the states of encreased and deficient excitement.

It may be necessary to remind the author that he has asserted, that every medicine operates as a stimulant. Although venœsection would not come within the definition of the word medicine, it is a means employed to restore the healthy state, and one might in effect affirm it to be a positive sedative power. It operates by an abstraction of stimulant power, without any stimulant operation, except the mere solution of continuity. Its effect in sthenic diathesis is greater than any other evacuation, and proves that an abstraction of all exciting power, agreeably to the true principles of this system, to be proper and advisable. Although the stimulant operation of evacuants is more than compensated by the discharge, they are hardly admissible on the sound principles of this theory.

There is a class of medicines called refrigerants, whose operation in inflammatory disorders is to abate excess of action. Extreme action of muscular fibres or of the arterial system, always generates heat, and as refrigerants, even when exhibited in so small a quantity as to produce no evacuation, appear to diminish excessive action; they therefore have a positive sedative effect.

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When diſſolved in water they increaſe the coldneſs of that element, and when adminiſtered in diſeaſe, their operation I ſuppoſe nearly ſimilar.

When given in health they counteract the ſtimulant effect of food and the other non-naturals, and diminiſh the temperature of the ſyſtem below the healthy point, which is what all phyſicians have meant by ſedative powers. The author's idea of a ſedative, is ſuch a power as would inſtantaneouſly deſtroy the vital principle. How far ſome poiſons might come within this deſcription of a ſedative power, every one may judge who has made any obſervations on their ſudden deliterious conſequences. Do they cauſe diſſolution in conſequence of a poſitive ſedative power? Or do they induce indirect debility? If the latter, their viſible operation does not manifeſt it: For as ſoon as any operation is perceivable, all the ſymptoms ſhew diminution of action; whereas, if a ſtimulant operation intervened between their exhibition and indirect debility, ſome ſymptom ſhould manifeſt it. If their ſtimulant operation is ſo ſudden as to inſtantly raiſe the excitement within the range of indirect debility, from what data can we infer a ſtimulant operation? We have no knowledge of medicines but from their evident effects on the body. Moſt gummy and reſinous ſubſtances have a ſtimulant effect, and by analogy we might infer, that poiſons poſſeſſing a gummy or reſinous part, would have a ſtimulant operation. But can we prove that they do not contain other principles

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which not only counteract the ſtimulant operation of the gummy and reſinous parts, but poſſeſs a power ſo ſtrongly ſedative as to induce immediate diſſolution. Even chemical analyſis of the component and elementary parts of a ſubſtance would not be an infallible teſt of their operation on the animal œconomy Some of the parts are diſſipated in the proceſs of ſeparation, and often when the ſeparate elementary principles are exhibited ſingly, they ſhew different effects than when exhibited in combination. If our medicines were directed to the excitement only, we need make no enquiries about the exerciſe of the different functions. The pulſe would be a certain index of the excitement, and all the buſineſs of a phyſician would be to encreaſe or leſſen it as occaſion required. Coſtiveneſs, ſtrangury, and deficient perſpiration, would no longer engage our attention.

The vital principle is the bond which connects all the functions together; but they are ſo independent of each other, that very often, while one is entirely vigorous and unimpaired, another is impeded in its exerciſe. It is alſo an eſtabliſhed truth, that in many diſorders of debility, where coſtiveneſs and many other functionary complaints exiſt, that they are not to be removed in any other manner than by the adminiſtration of a medicine ſuited to relieve that particular ſymptom. And although in its operation a cathartic may appear to increaſe the debility, it is attended with the moſt ſalutary effects, and is

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a neceſſary prelude to the ſucceſsful adminiſtration of ſtimulants. Suppoſe an infarction of the lungs to happen in a ſtate of evident debility, would a medicine of high or moderate ſtimulant operation, unaccompanied with the power of promoting perſpiration or expectoration, remove the infarction as certainly as a medicine of the ſame ſtimulant operation poſſeſſed of that power? I fancy the experience of moſt of the faculty will anſwer in the negative. And if it is true alſo, that by the exhibition of refrigerants and other medicines in ſthenic diatheſis, the diſeaſe will give way ſooner than to withdraw all nouriſhment and medicine, it then indubitably follows that theſe medicines poſſeſs a poſitive ſedative power.

Paragraph XCV. *In the cure of diſeaſes we muſt always ſtimulate or debilitate, never lay by, nor truſt to the ſuppoſed powers of nature which have no real exiſtence.*

That the human body contains within itſelf a principle of preſervation and renovation, is proven from facts ſo numerous and notorious that they cannot eſcape the attention of any perſon of obſervation. This opinion has been coeval with the firſt rudiments of medicine—& the principle is coeval with the embryotic ſtate of man. Its belief has been univerſal, and although the univerſality of its belief is not an abſolute proof of its truth, yet the numerous facts which corroborate and ſtrengthen it, make it a truth as undeniable as any in the ſcience of medicine.

This principle is diffuſed throughout all nature. Even inanimate matter ſeems to poſſeſs a principle of this kind. The attraction of coheſion exiſting in all matter, appears to be the ſame principle as the *vis medicatrix* in man.

What is it but this principle that cauſes the eyelid inſtantaneouſly to cover the eye whenever the leaſt danger approaches? What, except this principle, is it that cauſes the appetite of hunger to return at thoſe periods when the exigencies of the conſtitution require food and nouriſhment? We are not left to the ſlow dilatory deciſions of reaſon. In our appetites we are governed by inſtinct, and inſtinct is nature. The buſy avocations in which men are employed, would lead to a procraſtination of the neceſſaries of life, were they not impelled by the painful feelings of appetite. Is it not the efforts of nature that incarnates a ſolution of continuity? If our author anſwers food, I reply that a particular organization of the digeſtive and aſſimilating organs, and a particular contexture of the veſſels of the part, is the cauſa ſine qua non of this new growth of fleſh. The efforts of nature often reſtore perſons to health, without the exhibition of any medicine, and ſometimes in ſpite of medicine. It cannot be imputed to the operation of food; for it is well known that diſeaſed people generally have a diſreliſh and diſguſt to every ſpecies of nouriſhment. Admitting that they are what would ſupport health, in a healthy ſtate, it would not be equivalent to repair the waſte of excitement

excitement from diſeaſe. It may be anſwered that the exciting hurtful powers are removed. Pollutions of the atmoſphere, from the putrefaction of animal and vegetable ſubſtances riſing in the vaporous form, are aſſigned very juſtly as a powerful cauſe in the production of intermittent and remittent fevers; and perſons recover very often from fevers imputable to this cauſe without any medical aid, while others are daily attacked with the ſame complaint. This fact will not juſtify the inference, that the cauſe is removed. To what ſhall we aſcribe this fact, except to that principle in the conſtitution of man, whereby when any noxious powers are raiſed againſt the body, the *vis medicatrix* is excited to obviate the lethal tendency of thoſe powers. The diurnal revolution of fevers is a full confirmation of the exiſtence of the vis medicatrix. We ſhall not in this place diſcuſs the theory of fevers; but if ſuch a principle did not exiſt, would the cold, hot, and ſweating ſtages of fever, follow one another in regular meaſured order? Would a final ſolution take place without the adminiſtration of medicine? A ſtate of debility or encreaſed excitement on the principles of this theory, could never be recovered from. The ſyſtem, without medical aid would be condemned to perpetual and encreaſing diſorder until relieved by diſſolution.

The conſtant tendency to decline after the body arrives at its acme, the numberleſs noxious agents that exiſt without, would expoſe us to numerous infirmities, we would be conſtantly

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in jeopardy of death, we would have continual need of the phyſician, and the lot of man would be miſerable indeed.

Why does the ſtomach reject food when it is of a nature not calculated to nouriſh the body? Nauſea and vomiting are the firſt ſymptoms of the operation of poiſons. The ſame principle is perceptible in the brutal and vegetable creations. Animals often recover from diſeaſes without any aſſiſtance, and trees often decay in part without extinguiſhing the principle of vegetable life: Whereas, if no inherent power exiſted within to repel the hurtful agents, no reaſon can be aſſigned why the decay does not ſpread to the trunk as well as the branches. Why does the ſenſitive plant ſhrink from the touch?

The Doctrine of critical days ſo long obſerved and acknowledged by the faculty, is a confirmation of the exiſtence of the vis medicatrix.

In numerous fevers and pleuriſies that I have ſeen, I have generally obſerved a ſolution take place on an odd day, and very often by a conſiderable increaſe and evacuation of perſpiration, urine and the alvine diſcharge, which could not be imputed to the operation of medicines. Patients, when every medicine had failed, when they have been conſigned by the phyſician to the tyrant death, have ſuddenly recovered; an entire ſolution of the fever, and every ſymptom characteriſtic of the diſorder has diſappeared,

without any remaining ſymptom, except that of debility. If life was a forced matter, if the recovery was to be imputed to the force of medicine, a more gradual diminution of the diſorder would appear moſt natural. Facts ſo notorious cannot be overcome by the violent aſſeverations of Doctor Brown. The ſame principle ſeems to pervade the moral world. The affairs of ſtates and kingdoms, as it were by an invincible neceſſity, tend to a certain point, after arriving at which, by ſome inviſible cauſe, take a contrary direction. The diſorder of mania often proceeds from bad to worſe, until it arrives at a certain point, from which patients recover without aſſiſtance in a time much leſs than the time of its approach.

I am aware that apparently ſtrong objections may be raiſed againſt this principle. It might be aſked why the efforts of nature were not more likely to remove a diſorder in the beginning, when nature was ſtrong and the diſorder weak. I acknowledge it an unanſwerable objection. But how numerous and unaccountable are the phænomena of nature.

The ſame objection lies againſt the theory of the comets, either on the principle of Sir Iſaac Newton, or the more modern one of electricity. A comet is certainly more ſtrongly attached by the attracting body when in its perihelion, than in its aphelion; and yet in the former it is repelled, and in the latter attracted. When the

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influence of the attracting body would appear the leaſt, then the principle is exerted. This makes it more eminently an effort of nature. The *why* and the *wherefore* are often inſcrutable: But the fact is what we are to enquire after. The ſtahlian notion of two counteracting principles, diveſting the vis medicatrix of its rationality is not ſo chimerical an opinion. The deſtroying principle is the natural tendency of the body to decay, together with the external exciting hurtful powers; and the *vis medicatrix*, of Sthal diveſted of its rationality, is no other than the vis medicatrix of Dr. Cullen. On what the vis medicatrix depends, nor yet all the modes of its action, I ſhall not pretend to diſcuſs. I will obſerve, however, that the reaction of fevers, is undoubtedly to be imputed to the vis medicatrix, and as the ſpaſmodic or cold ſtage, ſeems neceſſary to produce the reaction, I therefore impute both the ſtages of fever to the efforts of the vis medicatrix. Contraction is a natural function of muſcular fibre. It is by this mode of action, that nature operates to expel the morbific matter of contagions and poiſons; and the object of the phyſician is only to moderate or encreaſe the contraction of muſcular fibre, as the exigencies of nature require. In the variola, why does it happen, that at a particular period after the approach of the ſymptoms, the contagion is caſt upon the ſurface? And why is it, that the efforts of nature expel the virus of the venereal diſeaſe without any aſſiſtance?

The body is not entirely paſſive under the operation

peration of hurtful powers. Were it ſo, we would fall an eaſy prey to their operation. Doctor Cullen, in his theory of the gout, obſerves that a balance ſeems to exiſt between the external and internal parts. The reſiſtance of muſcular fibres on the ſurface to the expanſive power of the heart, depends on their inherent power of contraction, aſſiſted by the preſſure of the atmoſphere.

Sir Clifton Wintringham has obſerved, that the proportional denſity of the arteries to the veins, encreaſes with the advance of age; and it may be noted that in the death of old age, that the denſity and reſiſtance of the arteries to the expanſive power of the heart, is ſuch as to overcome the latter. Although in ordinary health theſe two oppoſite powers are exactly balanced, nature has ſo framed the human body, that whenever the common reſiſtance to the expanſive power of the heart is increaſed, it indirectly excites a more than proportional energy of the heart and large arteries to overcome the reſiſtance.

Where this extraordinary energy of the heart exiſts, whether it reſides inherent in the muſcular fibres of the heart, whether there is a continual reſervoir of influence from which the heart is ſupplied, we will not affirm; but we obſerve that the vigor of muſcular contraction is in proportion to its contiguity or remoteneſs from the heart, the central point of action of the

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ſanguiferous ſyſtem. This is evident from the languor of circulation in the extreme veſſels.

The author of man may have for his preſervation, beſtowed on the muſcular fibres of the heart and large arteries, a more vigorous contractility, or have made them dependent on an unceaſing influence from the brain, the centre of the nervous ſyſtem, thereby to counteract the hurtful agents that ſurround the human race. We may obſerve the connexions and dependencies of the animal œconomy, but perfectly to unravel all its myſteries, ought not perhaps to be expected.

The human body differs from all machines ever invented by man. It contains within itſelf what has been an objeſt of curious inquiry and diligent reſearch by the moſt learned of every age and nation, "a perpetual motion." It is actuated by a vital principle, and the phænomena that it preſents to the curious obſerver, cannot be compared to the movements of any inanimate automaton. But ſuch a hoſt of facts conſpire to ſupport the exiſtence of the *vis naturæ*, that a man muſt cloſe his eyes upon the volume of nature; he muſt be deaf to the ſuggeſtions of reaſon and experience to deny its truth. We do not contend that the vis medicatrix acts uniformly, nor yet always ſucceſsfully. This would be to make man immortal. But in perfect conformity and conſiſtency with every animate and inanimate exiſtence, man contains within himſelf the ſeeds of death and life.

Paragraph

Paragraph XCVII. *The ſedative power of cold, is as ſerviceable in the meaſles as in the ſmall-pox.*

The good effects of cold in the ſmall-pox we preſume depends upon a peculiar operation on the ſkin. In the commencement of the eruptive fever, the application of heat is dangerous, on account of its debilitating the ſurface and thereby giving occaſion to a large eruption on which the danger of both meaſles and ſmall-pox may chiefly depend. The operation of cold appears to be partial.

The effect of vigorous contractions and efforts of the muſcular fibres is to generate heat, and therefore cold as a refrigerant, counteracts the hurtful and debilitating effects of heat; but in no manner does cold appear to diminiſh the energy of excitement, for if it debilitated the ſurface we would expect a large eruption. The effects of cold in different conditions of the body may be different. Cold and heat alternately ſucceeding each other, in moſt caſes increaſe the vigor of the healthy ſtate: can we therefore expect, that if cold increaſes the excitement when diminiſhed, that it will diminiſh it when increaſed. If ſo, it muſt be from an entire different operation in the one caſe from that of the other. Cold in our climate, when not too long continued, inſtead of debilitating, always invigorates. A ſedative operation and an increaſe of vigor, are incompatible. The good effect of cold then in the variola and meaſles, is from refrigeration, not poſitive ſedative operation.

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In a ſucceeding paragraph, our author imputes the good effects of cold in the ſmall-pox, to its relaxing the ſyſtem, but no phænomena of the operation of cold juſtify the opinion, except that in extreme degree, it occaſions gangrene. That it occaſions conſtriction of the ſkin, in ſome meaſure ſimilar to inanimate matter, is proven from the ſhrinking of the external parts, and the paleneſs or lividneſs that accompanies its application. I can by no means admit that cold relaxes the pores to give an eaſy free egreſs to the variolus matter, and thereby lighten the diſeaſe. It appears more probable that it conſtricts the ſurface and prevents the lodgment of the variolus particles under the ſkin. For where the reſiſtance to the influx of the blood into the cutaneous veſſels is taken off, the blood is urged forward with great impetuoſity and a crowded eruption appears.

The conſtriction or refrigeration that cold produces on the ſurface, detains the variolus matter within the veſſels until by their repeated action it is rendered inert. It is very improbable that cold or heat would ſo effectually relax the pores of the ſkin as to evacuate every particle of the variolus infection. Who would think of evacuating the venereal virus by an increaſe of the urinary or perſpiratory diſcharge? In what manner mercury acts to deſtroy the pernicious effects of the virus, whether as an antidote, whether by entering into combination with the virus and converting it into a tertium quid, or by ſtimulating

ulating the veſſels to ſubdue its nature, remains yet a ſubject of conjecture. But it appears plain from reaſoning, that if contagious matter is generally diffuſed in the maſs of blood, or only confined to the ſeroſity, that as an evacuation can neither diſcharge the whole maſs of blood with ſafety to life, nor yet the whole of the ſeroſity, the ſuppoſition of the contagion being evacuated through any or all of the emunctories is frivolous. That cold therefore operates by giving vent to perſpiration, and conſequently the variolus infection in the ſmall-pox appears equally frivolous. That cold is equally ſerviceable in the meaſles as the ſmall pox, I ſhall no further animadvert than barely to obſerve, that if the catarrhal ſymptoms in the meaſles originate from the diatheſis—it would appear rational to conclude that the removal of the diatheſis would be followed with the removal of the catarrhal ſymptoms. I will take occaſion to obſerve, that the alexapharmic mode of cure adopted by Doctor Sydenham, in the meaſles was exploded long before the publication of Doctor Brown's theory.

Paragraph CXII. *The ſtimulus of heat, in a moderate degree, produces its effect in due proportion, in a degree above that, the exceſs of its action is ſuch as to produce more or leſs of ſthenic diatheſis.*

I cannot ſuppoſe a degree of heat above moderate, can have exceſs of action. The author does not inform us in what manner he graduates temperature;

temperature ; but if we measure heat and cold by the same scale with which he measures disease, I could not believe that a few degrees of excitement above the point of health, would be followed with great excess of action. The natural temperature of the body is 98 degrees of Farenheit's scale, and the temperature of the air and other bodies that surround us are 62° of the same scale.*

Doctor Cullen thinks every degree of temperature below 62° has the effect of diminishing the temperature of the body, and is consequently hurtful—any substance on the contrary, possessing a temperature above 62° will raise the temperature ; from this point of temperature then we are to trace its effects on the body. The constant and continued effects of heat or cold on the body we know very little about, for neither heat or cold are so constant in our climate, as to produce effects uncombined with other causes. I would observe however, that the constant application of heat to such a degree as to occasion an increase of perspiration above the imperceptible vapor that constantly issues from the body, has a relaxing effect. This opinion is rendered probable from the leanness and emaciation that most people are subject to in warm weather. There is as Doctor Cullen observes, a certain degree of heat necessary to support the vital principle, and we will establish that temperature to be composed of the generating power of heat within the body,

*Vide Cullen's first lines, page 88.

and 62° the temperature of the bodies which surround us keeping the temperature of the body at 98°. We will admit for a moment, that heat augments the temperature of the body. We however deny, that it increases the tone and elasticity of muscular fibre—every appearance of the operation of heat, manifests a diametrically opposite effect. The effect of heat on the body by no means corresponds with the idea of increased excitement or phlogistic diathesis. An increased velocity of the pulse, an increase of perspiration, and a lassitude and weariness which accompany the application of heat, do not evidence an increase of excitement. In the exhibition of stimulant medicines, the momentum of the pulse is increased, vigor and activity in all the limbs and motions phænomena which we generally impute to an increase of excitement. Appearances so repugnant to each other cannot be imputed to identity of cause. By what law of the animal œconomy it happens that stimulus applied to the surface and stomach produce such opposite effects is very difficult to explain. If heat generated sthenic diathesis, why has not its use been enjoined to such a degree as to restore health in asthenic disease. Few physicians would be so lost and bewildered in theory as to advise heat in disorders of direct debility. In putrid fevers from indirect debility a gradual diminution of heat agreeably to the modus curandi of our author, would be most advisable; but if we may depend on the testimony of others, washing the body with cold water is not only innocent but salutary.

lutary. We are acquainted with no diſorders which may be ſaid to be the genuine and ſole effect of heat, except burns and ſcalds, which are the effect of the topical application of heat. They do not ariſe from a degree of heat barely above moderate, but from ſuch a degree of heat as never occurs in any ſtate of the atmoſphere; ſo that arguments drawn from their effect on the ſyſtem cannot be applied to diſorders ariſing from the heat of the air. Neither does it appear to me, that the operation of culinary heat is ſimilar to the heat of the atmoſphere. We ſhall not enter into the diſpute reſpecting the nature of fire, whether it be elementary or the effect of frictions. We do not conceive that the determination of this point would reflect any light upon the effects of heat on the human body. But we will obſerve, that heat ſimply conſidered, does not prove hurtful, ſo much on account of its altering the temperature, as in generating putrefaction in vegetable and animal ſubſtances, and thereby polluting and deſcompoſing the air.*

From a circumſtance related in the medical Commentaries of Edinburgh, of Doctor Monro, confining a number of perſons in a Bagnio, without ſuffering any great inconvenience until he raiſed the temperature to 210° of Farenheit's ſcale, it would appear that the operation of culinary fire is not ſo pernicious to health as the operation of an intenſe heat of the atmoſphere. The Siroc winds which blow every year in Sicily,

*Vide Cullen's firſt lines, page 186.

ly, are known to extinguish life instantly, & many hot days in the city of Philadelphia, are said to have the same effect when the heat of the atmosphere has not raised Farenheit's thermometer over 96° or 100°—To what can this difference be imputed except to the circumstance above mentioned. The effects of temperature on the human body are not accurately ascertained;—even Doctor Cullen's observations appear to me not sufficiently established. He defines the absolute power of cold, that degree which diminishes temperature, & every degree under 62, he considers as absolute with respect to the human body. I am disposed to believe however, that a degree under 62 would not if constantly applied, be attended with a constant diminution of temperature. Admit it to be a hurtful agent, the vis medicatrix would obviate its pernicious tendency, and the body would soon be accustomed to bear it. The effects of heat are when temporary confined to the skin, and do not alter the common temperature of the body. That heat unaccompanied with any other noxious agent is not very pernicious, is proven from persons sustaining a great culinary heat without any manifest inconvenience, while the burning of charcoal is often known to have the most deleterious effects. The intense heat of forges, and other mechanical employments do not produce disorders that are commonly the effect of the heat of summer. First, because it it not so constantly applied to the body. Second, because it is unaccompanied with marsh effluvia or any alteration in the atmosphere, except a diminution

nution of its denſity. Burning of powder, and other combuſtibles, are often uſed to purify the air. Any conſiderable diminution of the denſity of the air would undoubtedly be hurtful to the human body, as a certain denſity of it was intended by nature to aſſiſt the muſcular fibres of the ſkin and ſuperficial veſſels to counteract the powerful force of the heart in the circulation of the blood. But a ſmall rarefaction of the air is not attended with the hurtful conſequences to health that a leſs rarefaction would be when accompanied with an alteration in the component parts of the air. In what reſpect the particles of light that emanate from the ſun differ from the particles emited by a candle or culinary fire, I will not attempt to explain—but that they have different effects on the ſyſtem is to me a matter of firm belief.

Paragraph CXV. *Heat in extreme exceſs conſtantly debilitates. Its effect is ſomewhat greater upon the ſkin than in the interior parts, in which there is little change of temperature. Hence ariſe ſweating, as in the torrid zone, &c.*

That great heat ſhould diminiſh tone very little except in the ſkin, while a degree above moderate ſhould increaſe the tone and denſity of the muſcular fibres, every where appears to me a little aſtoniſhing. Whatever effect heat has upon the muſcular fibres generally, muſt be in conſequence of a ſympathy exiſting in all parts of the body. And although I do not believe that

a change in the temperature of the skin is so readily communicated to the internal parts, as a change in the condition of the stomach is communicated to the skin, or other parts; yet I would suppose that if a small change in the temperature of the skin would be communicated to the distant parts a greater change would be more speedily and effectually communicated. The author in account of temperature, seems to be something inconsistent. For in a succeeding paragraph he says, the same agent (meaning heat in extreme excess) *in the violent measles, in the confluent small-pox, in fevers, and in every asthenic disease, in which the perspiration is deficient, though it expands the vessels increases deficiency of perspiration.*

How the same power produces two opposite effects, the author has attempted no explanation. If, however, health and disease are the same states, the effect of heat in health and disease would be the same. But as two opposite effects are induced by the same degree of heat, we must infer that health and disease are not the same. Heat and cold are relative terms, and it would be well if we could fix upon a middle salutary point of temperature, from which to reckon both the morbid and salutary effects of heat and cold. It is a task beset with many difficulties. Our author's attempt is very indifferently executed, and although my observations on temperature may not appear very methodical, I hope they contain some useful truths. Doctor Brown has certainly misrepresented the effects of temperature on the human body. The degree of heat above moderate, I cannot suppose to generate

ſthenic diatheſis. The known healthineſs of the inhabitants of northern climates, and eſpecially in the coldeſt weather, does not correſpond with his imputed effects of cold.

Paragraph CXX. *As cold is naturally debilitating, and all debilitating powers diminiſh excitement, it is therefore never of ſervice but in ſthenic diſeaſes.*

The temperature of heat and cold, as long as it keeps within the range of agreeable ſenſations, is no doubt ſalutary. But when it exceeds that range conſiderably, and is conſtantly applied, it then becomes hurtful. As the ſenſation of cold or heat however, is greatly diverſified, owing to the ſenſation immediately preceding, it is difficult to eſtabliſh what degree of temperature is agreeable. We are the creatures of habit, both in our moral and ſenſual feelings, and a degree of temperature that is agreeable to day, may tomorrow become diſagreeable. The extremes of heat and cold produce pretty nearly the ſame effects on the human body. But if we fix upon a middle point between two extreme oppoſite points, I doubt whether a diminution of temperature, by which we mean cold, would be ſo hurtful as an increaſe of temperature. The intermediate degrees of temperature in their effects, do not approximate ſo nigh one another as the two extremes. Arguments drawn from the effects of cold on vegetable life and the elements, very illy apply to the living ſyſtem: For

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as the human body contains a power of generating heat within itself, a degree of cold that would extinguish vegetable life and destroy the form and consistence of the elements, would, by proper successions and alternation with culinary heat, be salutary.

Experiments have discovered that some kinds of air that are the most powerful in extinguishing the vital principle in animals, increase and cherish the growth and perfection of plants.—The different organization of animal and vegetable life, modifies the operation of temperature. Cold has a different operation on the brute creation from what it has on the human species. Animals enjoy better health in the summer, are more fat and plump, while the human race emaciate, and are more diseased than in winter. Whether this may be imputed to the alternation of cold with heat, or to a weaker power in generating heat in other animals than in man, I will not pretend to determine. The fact that some vegetables live all winter, proves that the noxious or salutary power of cold depends on organization as well as habit. A fact that proves cold less prejudicial to health is, that nature in the formation of man, did not limit the vital principle in generating heat. Whenever the vital principle is strong, a great quantity of heat will be generated, and even when it is apparently weak, a nisus will be exerted to restore the health of the system. The constitution of man being thus formed, it was necessary that some external

external agent should exist whenever redundancies of heat were accumulated to refrigerate the body and reduce it to the healthy standard of temperature. In atmospherical air, we find an agent every way suited to produce this effect: and I agree so far with Doctor Brown as to believe in the utility of cold in sthenic diathesis, but I do not agree to its being so dangerous in disorders of direct debility. Doctor Cullen observes, that cold manifestly possesses a tonic and astringent power in its operation on the body. He also observes that every change of temperature from higher to lower, proves more or less stimulant, except it is so intense as to extinguish the vital principle in the part. The first effect of the application of cold, he observes, proves both astringent and stimulant. These observations are founded in truth. Even in cases of direct debility the application of cold when not long continued, is salutary. Convalescents recovering from fevers of every kind scarce ever return to their former vigor, until they have experienced the stimulant and tonic effects of cold. Cold bathing has been recommended in intermittent fevers and the debilitated state of patients recovering from every disorder. And although it may be well to be governed in advising this mean by the causes which produced the disorder, and the species of debility that exists; yet the hurtful effects of cold I am not very apprehensive of, except a degree of cold below 62° and that degree constantly applied, or some obstructions exist in some of the viscera.

In short the sedative effects of cold so much feared by Doctor Brown, and spoken of by Doctor Cullen, as taking place from every degree of absolute cold, appears to me to exist in idea rather than in fact: For except cold extinguishes the vital principle, induces a palsy or gangrene, all the other effects evidence a stimulant operation.

Doctor Cullen, in his account of the sedative and stimulant power of cold, delivers what appears to me an inconsistency:—For he says, the sedative power of cold takes place from every degree of absolute cold; and yet, in a succeeding aphorism, he says, every application of temperature which gives a sensation of cold, whether from the absolute and relative powers of cold, is in its first effect both astringent and stimulant. I entirely coincide with Doctor Cullen, that the first effect of cold is to stimulate in every condition of the system, except in sthenic diathesis, and then if constantly applied, it manifestly refrigerates and lowers the temperature. Its refrigeration even then depends on its constant application; for if it was alternated with culinary heat in the small-pox, it would only serve to encrease the diathesis. In addition and confirmation of what I have said respecting the difference between culinary heat and the heat of the sun, it is my opinion, the reason why a colder temperature agrees better with the constitution than a hotter is, that with the former the density of the air is intimately connected, and with the latter its rarefaction.

It

It appears to me that a perſon expoſed twelve hours to a conſiderable degree of cold, and then expoſed to twelve hours heat of a ſummer's day, would feel more debilitated than to have experienced a degree of culinary heat equal to the heat of the ſun.

I am ſenſible of the want of connection in this view and account of the effects of temperature. It is a ſubject attended with great difficulties in its diſcuſſion.

Paragraph CXXXIV. *It is not an exceſs in the quantity of blood that upholds hæmorrhages, but laxity and atony from its deficiency.*

It appears a matter attended with great difficulty to eſtabliſh the pathology of hæmorrhages on any general ſimple principle. They occur in ſuch different conditions of the ſyſtem, that I have no doubt in ſome inſtances they may be owing to debility, and in others phlogiſtic diatheſis. Although Doctor Cullen arranges this order among his pyrexial claſs, we cannot infer merely from his arrangement, that they are the ſole and conſtant offspring of inflammatory diatheſis. They reſemble the other orders of this claſs in being attended with an original fever. He ſuppoſes the ſpaſm and fever formed prior to the appearance of the diſcharge. Although it appears from his explanation of the ſubject, that the *vix medicatrix* is inſtrumental in their production, I have ſome doubts about the originali-

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ty of the fever:—For in the ſame order he has arranged pthiſis pulmonalis, the fever of which he denominates ſymptomatic. The foundation of this diſtinction is not ſo obvious. Let the fever be primary or ſymptomatic, a partial debility or unequal excitement ſeems neceſſary to precede their appearance. The effects of food and the other non-naturals on the animal œconomy often elude inveſtigation. It is poſſible that the non-naturals may have ſome agency in producing a partial debility, but if ſo, it is in ſuch an inſidious manner as to eſcape medical ſagacity. The parts ſubject to hæmorrhages by not being covered with the ſame thickneſs of ſkin as other parts of the ſyſtem, might naturally be offered as a reaſon for their ſo frequent occurrence in thoſe parts. They are more liable to be effected with the viciſſitudes of the air. But we would preſume a priori, that the coats of the veſſels of thoſe parts would have been formed more denſe, to reſiſt an influx of blood ſufficient to produce a rupture. Experience, however eſtabliſhes that they are more ſubject to hæmorrhages than other parts, and we may juſtly impute their frequent appearance to this cauſe. Hæmorrhages very ſeldom, if ever, occur in infancy or advanced age. They generally happen at the period of commencing manly vigor, when large quantities of food are taken in, and the digeſtive organs are vigorous. They may poſſibly occur in ſubjects labouring under debility, but it is my ſettled, rooted opinion, that it is a rare occurrence. Why do they not happen in debilitated ſtates of

fevers,

fevers, dropſy, and other diſorders of debility? They ſometimes happen in the firſt ſtage of a fever, and I have generally remarked that their occurrence at thoſe times was ſalutary; whereas, if they were the offspring of debility, they would have been morbid. Although diſcharges of blood do frequently happen in different parts of ſcorbutic patients, I have never known of an hæmoptyſis occurring in that diſorder. Why do they not happen in patients recovering from fevers, when a greater apparent debility exiſts than when the fever rages with the greateſt violence? I impute it to the ſtrong inflammatory exertions of the animal œconomy. Let the opinion of the faculty be what it may, I am confident that in every paroxyſm of a fever, the ſtate of the ſyſtem reſembles inflammatory diatheſis. What adds conſiderable weight to the opinion that hæmorrhages depend on a ſthenic ſtate of the ſyſtem, is that they are apt to produce a plethora of the ſyſtem. I have ſeen young girls labouring under an hæmoptyſis, exhibit ſymptoms of the higheſt health, a florid, full countenance, and every mark of redundancy of blood. In conſequence of this plethoric ſtate of the ſyſtem, a periodic diſcharge would every now and then happen. This plethoric ſtate of the ſyſtem Doctor Cullen accounts for very ingeniouſly, by ſuppoſing that the exhalants, ſecretory, and excretory organs, in conſequence of a diminiſhed quantity of the maſs of blood, are not filled with their proper fluids. The veſſels collapſe, become rigid, and reſiſt a

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very copious entrance of fluids, whereby they are detained in the ſyſtem and a plethora follows, and conſequently a diſcharge becomes natural and habitual. They occur in ſeaſons of the year moſt apt to generate an abundance of blood. As far as I have been able to learn, they more generally occur in the ſpring ſeaſon than any other period; and from this circumſtance we may infer, that the tonic power of cold, increaſing the tone of the muſcular fibres has, by invigorating the digeſtive organs, accumulated a copious quantity of blood in the ſyſtem.

The warmth of the ſpring, by deſtroying in ſome meaſure the elaſticity and denſity of the air, and that by impairing the tone of the ſolids and veſſels of the part, gives occaſion to their appearance. Perhaps (as Doctor Brown ſays) the tone of the veſſels of the ſkin is increaſed during the winter more than any other part, they may retain their vigor and tone in the ſpring longer than any other part; and as there happens to be an accumulation of blood in the ſyſtem, and as the veſſels of the ſkin reſiſt its influx into them, it muſt be determined into veſſels poſſeſſing leſs reſiſtance. Women who from ſudden colds are troubled with a ſuppreſſion of the menſes, often have a vicarious diſcharge of blood from the lungs; and as the catamenial diſcharge when ſuppreſſed, is a redundant quantity of blood detained in the ſyſtem, a plethoric ſtate muſt naturally be ſuppoſed to exiſt: and this affords a ſtrong argument againſt debility as

the cauſe of hæmorrhages. I will not anſwer for the pathology of Doctor Cullen, but I deny the truth of Doctor Brown's.

What agency the *vis medicatrix* has in their production, I will not determine; but if it has any agency, it is leſs conſpicuous than in fevers, and eſpecially as Doctor Cullen has pointed out no remote cauſes to induce debility or encreaſed tone, the exciting cauſes of the operation of the *vis medicatrix*. It is a ſettled opinion of mine, that the exertions of the *vis medicatrix* are always determined from the centre to the ſurface, and almoſt always in increaſing ſome evacuation. We have, however, heretofore admitted that the cold ſtage of fevers was a part of the operation of the *vis medicatrix*. We meant it only as a neceſſary mean to excite the action of the heart and arteries, to reſtore the debility exiſting on the ſurface. And we will not ſay that a ſpaſm in hæmorrhages may be formed on the ſurface, on purpoſe to excite the action of the heart and arteries to throw off by the excretories the redundant blood accumulated in the ſyſtem. The failure of the *vis medicatrix* muſt be imputed to the obſtinacy of the ſpaſm determining the blood to the lungs, or ſome part poſſeſſing leſs reſiſting energy. The diſciples of Doctor Brown muſt not expect the *vis medicatrix* to be always ſucceſsful in its efforts. If it is ſucceſsful in a majority of caſes only, it is ſufficient to eſtabliſh the principle. One caſe of hæmoptyſis occurs to me, which cannot be imputed to the

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agency of the *vis medicatrix*. Perſons in appa-rent health have been ſaid, upon aſcending the Peek of Teneriffe, to be ſuddenly attacked with an hæmoptyſis. Can the rarefaction of the air on that ætherial mountain occaſion ſuch a ſudden loſs of tone and vigor as to excite the *vis medica-trix* into action?

The ſtimulant plan of cure in hæmorrhages, I entirely diſapprove. They encreaſe the morbid activity of the veſſels; increaſe the local inflam-mation which always exiſts in hæmoptyſis, what-ever be the ſtate of the general ſyſtem. When-ever a diſcharge has become ſo abundant as great-ly to weaken the body, we are neceſſitated to have recourſe to ſtimulants and tonics. And when, from the long continuance of the diſ-charge, the pulſe becomes ſmall and nature be-gins to flag, the neceſſity of their adminiſtration is obvious. But in the beginning they are ex-ceedingly detrimental.

Practitioners have been deceived with reſpect to the utility of ſtimulants in hæmorrhages. When the patient has become weak and emacia-ted, ſtimulants have ſeemed to reſtore vigor, and an abatement of the ſymptoms have follow-ed. But it is only to return with more vigor. I conſider it as a deceiving appearance both to the phyſician and patient. It cheriſhes hope, the laſt comfort of the wretched, only to depreſs them the more. This idea is ſubſtantiated by the evidence of Doctor Brown himſelf. He re-

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lates a case of a person under the cure of one of his pupils, labouring under a pthisis, who by a voyage to Lisbon and the administration of stimulants, received a cure. But two years afterwards death overcome the skill of the physician. The same may be said of the stimulant practice in pthisis pulmonalis. I have uniformly seen that practice not only fail, but often attended with an aggravation of every symptom. It increases the stricture of the breast, aggravates instead of checking the violence of the fever. I have always made it a rule in the exhibition of the cortex, if it did not entirely stop the fever after giving it a sufficient length of time, and in sufficient quantity, to lay it by altogether; and I always conclude, that when it does not have a salutary effect, some morbid condition exists in the whole system, or in some part of it except debility. As the nervous influence seems to pass from the brain to the extreme parts, perhaps a solution of continuity may interrupt its progress, raise a commotion in the part, and thereby impair or change the operation of stimulants. I hazard this conjecture, however frivolous it may appear to the disciples of Doctor Brown.

One chief circumstance affecting the operation of medicine in hœmoptysis, is the inspiration of air into the lungs. The other discharges of blood are easily cured; but all the art of man has never been able to find an infallible remedy in hœmoptysis and pthisis pulmonalis. The dis-

covery of ſpecifics, I believe is a chimerical expectation. For the condition of the body is ſo different in the ſame diſorder, that a remedy applicable at one would not be equally ſo at another period. Man ſeems born to deception; they are dupes to appearances, and in no inſtance has it been more conſpicuous than in the work under diſcuſſion.

Paragraph CLXXX. *If at any time the pulſe becomes full and hard without a proportional relief of the ſymptoms, it is a bad ſign, and happens becauſe the ſtimulant plan has been puſhed beyond the rule, and is a caſe of indirect debility ſuperadded to the direct.*

What ſtimulants are we poſſeſſed of that will raiſe an aſthenic diſeaſe inſtantly to the point of indirect debility? Will not the excitement in paſſing from one to the other, paſs through the point of health? Is it not an abſurdity to ſay that indirect debility exiſts during the exiſtence of a hard full pulſe? I have ſeldom, if ever obſerved ſuch an appearance, and were it to happen, it is much eaſier to leſſen the pulſe than to raiſe it. This is one of the new opinions of this miſleading theory. Our author often ſtumbles upon the dark mountains of error.

Paragraph CLXXXIX. *Wine, aromatics and volatile alkali, and above all the reſt, the various forms of opium, diſlodge from its ſeat all ſuch hurtful matter, without either vomiting or purging, and without difficulty, and in a ſhort time.*

The administration of emetics and cathartics in collections of crudities in the stomach and intestines, is a practice sanctioned by the experience and authority of ages. I am confident that wine and aromatics, and especially opium, instead of discharging crudities in general, serve to aggravate and procrastinate the disease. We may aptly here apply an observation of Doctor Cullen in speaking of the effect of cathartics in discharging hydropic waters, that if they do not suddenly have that effect, they increase debility. This is a general principle pervading the animal economy. Opium, on account of its constipating the bowels, seems the most improper of all stimulants. It suddenly abates pain, and where no crudities are collected, and the pain depends on simple debility, it may by repeated exhibitions effect a radical cure; and I imagine it has been cases of this kind that our author has mistaken for collections in the elementary canal. And although I agree with the author in supposing them the effect of debility, I cannot believe in simplifying medicine so far as to raise but one indication of cure. The muscular fibres, as the author says, differ from common elastic matter in this; that they contract not only when the distending power is removed, but even while it remains; But they do not contract with that facility in the time of distention, as when the distending cause is removed. Ineffectual efforts in the muscular fibres are often made to contract, and the consequence is an encrease of debility. And although I may not be able to explain why stimulants fail

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of discharging the crude contents of the bowels, I am certain of the fact; and no theoretical reasonings will ever induce me to give up this belief. Crudities collected in the primæ viæ may prevent medicines coming in contact with their muscular fibres. An unequal excitement undoubtedly exists in those cases, and the failure of stimulants may be imputed to this source. I am clearly of opinion, that to the successful administration of tonics and stimulants, a uniform exertion of the vital principle is absolutely necessary. It is also necessary that previous to their exhibition the secretions and excretions should be restored to their usual quantity.

It may be a questionable point with some whether stimulants or diaphoretics are the most certain to restore the excretions. But experience has confirmed me in the belief that medicines of the latter class, whether they effect a cure by a stimulant or sedative operation, are more effectual than medicines without this property possessing a higher stimulant power. And what strengthens this opinion, and especially the observation that the success of stimulants and tonics depend on the uniform exertions of the vital principle is, that I have always observed their exhibition to be most successful when perspiration was restored, and the patient entirely free from pain. Although spasm (as the author says) may be a deficient function, yet as it is a nisus to discharge the crudities, or to restore the debility, the operation of stimulants often aggravate

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the niſus without effect and increaſe the debility. The activity of the fibres are often too vigorous, and like the reaction in fevers, require to be abated previous to the exhibition of tonics.—Since Doctor Brown's ſyſtem made its appearance, I have in ſome inſtances attempted his mode of practice, but the ſucceſs which he flatters us to expect, entirely failed me in the trials I have made. I had under my care a child laboring under diarrhæa from weaning, and from the emaciated ſtate and age of the patient, it appeared to be a caſe in which the ſtimulant method of cure bid as fair to be ſucceſsful, as any that falls under common obſervation. Previous to my determination to exhibit ſtimulants, I had uſed Rhæi without any effect. I then exhibited brandy, and to make it a little more ſtimulant, I added a ſmall quantity Canella alba, with the ſame ſucceſs. I then began to exhibit liquid laudanum twice a day, increaſing the doſe from day to day, but without the leaſt abatement of the diſeaſe. Diſheartened in the uſe of ſtimulants, I reſorted to an emetic. Immediately after the operation of the emetic the ſtools became leſs frequent and of a thicker conſiſtence. Encouraged with the ſucceſs of the firſt, I exhibited a ſecond emetic. It operated in a moſt violent manner, both upwards and downwards, ſo much ſo, that from the emaciated and debilitated ſtate of the patient, I began to fear the roughneſs of its operation might be dangerous, but with the exhibition of a few drops of laudanum the vomiting and purging ceaſed, and from that time

the diarrhæa abated, the child recovered appetite and flesh, and the cure was complete and effectual. From this and other cases which have fallen under my observation, I am perfectly convinced of the impropriety of following Doctor Brown's general *modus curandi*. Cases may exist where no crudities are present, and where the *vis medicatrix* may be wearied so that the fibres become almost entirely passive, and in which his mode of cure may sometimes be successful. I have seen alarming appearances in the dysentery from the continued exhibition of opium. And what would be the consequence if his practice was universally imitated. Pain and spasm do not always arise from debility, together with a distending matter. A state of emptiness in the stomach and intestines, occasions the painful sensation of hunger.

Paragraph CXC. *Pain in the external parts, also depends upon spasm, but not with the conjunction of distending matter, and a power takes the place of it, which is not to be referred to any matter, but to a certain effort of the will in moving a limb.*

In what manner an effort of the will can act as distending matter, is to me entirely incomprehensible. Can it be an instinctive exertion of the will to remove pain, which is conspicuous in patients tossing and changing their position, when labouring under spasmodic affections? This happens after the commencement of pain, and there-

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fore cannot be said to operate in its production. This would countenance one of the greatest philosophical absurdities, that the effect existed before the cause. From the known effect of pain the author infers that the will operates in the external muscles in the production of pain, as a certain portion of excrement would were it to exist in a muscular fibre. Fanciful indeed is the supposition, and (as the author says) contemptible are the resources of ignorance. It may cherish in the mind of the author a strong opinion of the novelty of his work; but it will answer no useful purpose in the illustration of the pathology of disorders.

In the same paragraph we have the following remarkable sentence—"*Spasm in the external muscles is removed by restoring strength; for that reason the cause also must be the same and be reducible to debility, together with something that resembles debility and possesses power equal to it.*"

What effort of the will resembles debility? Can debility be compared to any thing in nature except the relaxation of a simple cord? Strong exertions of the will produce debility. But the cause and effect have no resemblance. We associate cause and effect; but the association is not from resemblance, but as prior and posterior.

Paragraph CXCI. *The pain of cholera arises from a concentrated acid predominant in the alimentary canal.*

That

. That there conſtantly exiſts a portion of acid in the alimentary canal, and that the quantity may be increaſed in a debilitated ſtate, will be readily admitted. In caſes of an increaſed quantity of acid in the primæ viæ, its power of ſtimulating the fibres in a given quantity may be enlarged; but we have no poſitive proof of it, for the pain may with great propriety be aſcribed to the enlarged quantity, not to any concentration of the acid. It is incompatible with the idea of a centrated acid to ſuppoſe the quantity increaſed. A concentrated is a diminiſhed quantity of acid, poſſeſſing the power of the quantity from which it is diſtilled. The debility of the inteſtinal canal, does not operate in the production of acid any way ſimilar to chemical operations upon acids ſubjected to chemical proceſs. If it is neceſſary to diſtinguiſh this from other acids, it would bear the name of a diffuſed better than a concentrated acid. In this diſordered ſtate of the inteſtinal canal ſtimulants and tonics, often fail after the evacuation by vomit and ſtool, have proceeded ſo far as greatly to debilitate. A powerful conſtriction may exiſt on the ſurface, determining the fluids with great impetuoſity to the ſtomach and inteſtines, to which may be imputed the failure of ſtimulants. For we are often neceſſitated to have recourſe to external means, as epiſpaſtics and warm bathings before ſtimulants have a ſalutary effect. I have ſeen caſes of cholera, after the ſtrongeſt ſtimulants failed, give way to Magneſia and Columbo, ſubſtances which poſſeſs no great ſtimulant power. They act as abſorbent, rather than ſtimulant. Numerous examples might be

adduced to disprove the notion of one indication being sufficient to effect a cure in disorders of debility.

Paragraph CXCIV. *As an acid produces pain in the internal parts, so in the external parts, it is occasioned by something that produces the same effect as the acid that depends upon the will and acts in conjunction with the convulsive state.*

What is that something that depends upon the will, acting like, or producing the same effect as an acid? If it depended on volition, why are we not instructed how to remove it by an effort of the will? I cannot conceive of a modification of the will acting like an acid in the production of pain. It is as mysterious and enigmatical as the Sybilline or Delphian oracles. I will not admit that he has discovered upon scientific principles, a certain mode of cure for one half of the disorders of the human race, while former physicians have universally failed. I really wish to imbibe a small portion of this effulgent discovery, but whether it is to be imputed to the dullness of mental vision, or renitency to unlearn what I have already learned, I will not, cannot determine.

If the author has any disciples in America, I call upon them to develope and illustrate principles so important to the human race. In his review of morbid affection, the author traces the symptoms in the order in which they make their

appearance. Want of appetite, naufea and vomitting, are by no means the certain forerunners of every diforder. Diforders often exift without any apparent change in the ftate of the ftomach. The effects of drinking cold water are as various as the different conditions of the animal œconomy. I cannot fuppofe it very debilitating; on the contrary I have generally obferved it to have a tonic effect upon the mufcular fibres of the ftomach, and by its fympathy with other parts, a like effect upon the general fyftem. In fevers, phyficians have generally been cautious of a too free indulgence in draughts of cold water, but I imagine the caution did not proceed from its debilitating operation. If we may depend on the reports of others, I have no doubt that cold water has effected an entire folution of fevers. The mode of its operation may be myfterious, but I am fure it is not by increafing debility, *the cauſe of fevers.*

The fears of practitioners in the ufe of cold water are every day diminifhing. Their fears have often been groundlefs, and I am happy to fee this fymptom of freedom of thought and inquiry among the faculty, who have heretofore been governed more by whim and caprice than found judgment and reflection. The operation of cold water is moft doubtful in pneumonic inflammation. It has been obferved to occafion pleurifies when taken in large quantities, and efpecially when the circulation has been quickened, and the heat of the body has been raifed confidera-

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bly above its ordinary temperature. This effect could not be imputed to the ſedative and debilitating operation of cold water. The faculty in the cure of the colic, have generally been in the habit of adminiſtering cathartics, and eſpecially when attended with conſtipation of the inteſtinal canal. Colics are ſometimes of an inflammatory nature, and at others they appear to be the offspring of debility. And as thoſe two different conditions of the body would not alike be removed by the ſame means; an uniform adminiſtration of ſtimulants would not be juſtifiable nor ſucceſsful. Whenever the healthy tone of the inteſtines is impaired, coſtiveneſs or laxneſs generally ſucceeds. And when ſpaſm, pain and coſtiveneſs have happened together, phyſicians, without any regard to encreaſe or diminution of tone, have began the cure by an evacuation of the contents of the bowels. And although Doctor Brown ſays, that an invigoration of the inteſtinal canal will effect a certain cure without this precaution, I doubt the truth of the aſſertion. In ſlight affections of the bowels unattended with coſtiveneſs, an opiate may affect a radical cure. But when the belly is obſtinately coſtive, no invigoration of the periſtaltic motion will enable the inteſtines to diſcharge their contents. Admitting ſtimulants would have this effect, it appears to me that premiſing a cathartic would expedite inſtead of procraſtinating a cure. The diſtention of the fœces oppoſe the contraction of the inteſtinal fibres, and as it is by contractions their healthy vigorous ſtate is reſtored, the ſpeedieſt

dieſt way will be to evacuate their contents, that the muſcular fibres may have free liberty to contract. Doctor Brown certainly did not urge the uſe of ſtimulants ſo much with a view to ſave time, as to impreſs the world with the amazing ſtrength and fertility of his genius and novelty of his ſyſtem. From manifold experience, I am convinced that opium, although it abates pain in colics, yet as it encreaſes coſtiveneſs, after its operation is taken off, it diſpoſes the inteſtines to fall into ſpaſms, and an effectual cure ſcarce ever happens, except the bowels are firſt evacuated. Nor have I more faith in the uſe of ſtimulants in tabes, atrophy and worms. Atrophy and tabes are ſometimes owing to obſtructions in the meſenteric glands, and ſometimes I believe they may be imputed to an abundant quantity of mucus lining the inteſtines of children, cloſing up the mouths of the lacteals and thereby preventing the abſorption of nutritious matter. But to whichſoever of theſe cauſes they are owing, I doubt whether ſtimulants poſſeſs ſuch extraordinary power as to remove theſe complaints in a few hours, days or weeks.

An atrophy or tabes from obſtructions in the meſenteric glands, is a diſorder of the moſt confirmed obſtinacy. I have very little experience in either of theſe diſorders, but the reports of others, on whoſe veracity I rely, inſpire me with a belief that they are very little under the power of medicine, and leaſt of all under that claſs of medicines called ſtimulants, they confirm the obſtruction and aggravate the diſeaſe. When

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these complaints originate from mucus in the intestines, stimulants may by exciting vigorous contractions of their fibres, hasten their discharge and consequently a cure. But opium, one of his diffusible stimulants certainly diminishes the activity of the muscular fibres and would therefore appear a very improper medicine in this and many other complaints in the use of which Doctor Brown is so liberal—I have the same doubts about the use of stimulants in worms. A small use of spirits or some stimuli are of use to keep up the tone of the bowels of children—they are disposed to be lax and flaccid, and their secretions are greater in proportion to the mass of blood, than adults; but I am positive that a continued use of stimuli for weeks and even months, would not dislodge these vermin so effectually as Dianthis and calomel in a few days. And I am confident that physicians will not give up a sure and certain cure in Dianthis and calomel upon the *ipse dixit* of Doctor Brown. Worms are more rare in Europe than in America—And whatever purpose stimulants may answer there, I am sure it would be madness for the faculty in America to lay aside a sure and effectual cure for one that stands on the feeble foundation of an individual's word. We may and very often do have recourse to stimulants and tonics to brace and fortify the bowels against their future generation.

Paragraph CCVI. *There are four kinds of inflammation, two universal, a sthenic and an asthenic, two local a sthenic and asthenic.* The

The aſthenic inflammations are creatures of imagination; they do not exiſt in rerum naturæ. Inflammation conveys an idea of a tenſe elaſtic muſcular fibre through the whole ſyſtem or in local inflammations a vigorous action of the veſſels in the part and an abundance of blood in the habit. The idea is in direct oppoſition to an aſthenic ſtate. The epithet aſthenic when conjoined with the word inflammation is incongruous and incompatible—they import contrary ideas. The adjective ſhould always agree in ſenſe with the ſubſtantive to which it is affixed. Inflammation is ſynonymous with increaſed excitement—It is as abſurd to ſay aſthenic inflammation, as to ſay commendable or good vice. They convey an idea of two oppoſite conditions of body exiſting at the ſame time which is impoſſible. Our author poſſeſſes a happy faculty of reconciling ideas which in every other perſon's mind ſtand in direct oppoſition. The affection which he denominates a general aſthenic inflammation in the gangrenous ſore-throat agrees no further with inflammation than in the bear redneſs of the fauces. The veſſels of the part appear languid and inactive; they manifeſt no appearance of increaſed vigor; there are no forcible contractions of the fibres; no violent pulſations of the arteries appearances, eſſentially neceſſary to conſtitute inflammation. In a former part of his work he has aſſerted that all the ſymptoms are participant of the nature of the diatheſis. The diatheſis in the putrid ſore-throat is aſthenic, and as the word aſthenia is oppoſed

posed to inflammation, therefore an inflammatory state depending upon the diathesis cannot exist. The fluids in this disorder are determined to the throat in larger quantities than any other equal part. In what manner the effluvia of this complaint affect the throat more than any other part of the system is inexplicable. Can we suppose that the first application of the contagion is exerted upon the fauces, and from thence communicated to the system? Or do they exert an operation upon the whole surface of the body? Is the more violent affection of the throat owing to the want of the cuticle? He advises the exhibition of powerful stimuli to drive on with impetuosity the blood which loiters in the affected part, but if the medicines administered had not the effect to brace the whole system and the vessels of the affected part in equal proportion, they would urge the blood only to increase the debility of the vessels of the fauces. For the first days of this disorder it often exhibits an inflammatory appearance, but it is a deceitful appearance—the pulse does not shew that vigor that is so conspicuous in pneumonic inflammation; and as an inflammatory diathesis, must always precede a state of indirect debility, and as the first stage does not shew an increased excitement sufficient to induce indirect debility, we cannot infer its existence. In states of indirect debility suddenly brought on while the vessels are yet full, I am convinced of the propriety of venœsection and other evacuations, thereby to give the vessels liberty to contract. Venœsection is rarely admissible

ſible in a complaint ſo ſtrongly marked with debility and putrefaction. I can ſee no reaſon for ſuppoſing this complaint ſymptomatic, any more than the cynanche tonſillaris. They are both ſignificative of the diatheſis on which they depend. I agree with the author in believing this complaint improperly arranged in the ſyſtems of noſology. The affection of the throat, and the general condition of the body is ſo different from the other affections of the throat with which it is arranged, that its claſſification appears entirely prepoſterous.

Paragraph CCXXV. *The burning ſenſe of heat on the palms of the hands and ſoals of the feet, is owing to debility checking perſpiration, becauſe fatigue, cold and other debilitating powers are hurtful, and heat, reſt and ſtimulants are ſalutary.*

This painful ſenſation is undoutedly owing to debility, becauſe I have always experienced whenever that affection came on, that cold air always removed it. Cold I conceive operates in this inſtance by diminiſhing heat and bracing the weakened parts. Fatigue and heat always increaſes this ſenſation. Whatever effect cold may have in the natural ſtate of the body, I am ſure that when an extraordinary degree of heat is generated in the whole ſyſtem or any particular part, whether from cauſes increaſing or diminiſhing the tone of the fibres that cold by refrigeration diminiſhes the heat and reſtores the healthy ſtate.

Paragraph

Paragraph CCXXX. *If fever, if the gout indigestion, colic, asthma and all asthenic diseases have lately, to the conviction of every person who gave the subject a due consideration been proved to yield to the various forms of opium without difficulty can we suppose it proves of service by a debilitating operation?*

The operation of opium is involved in great difficulty. The notion of its being stimulant is not the offspring of Doctor Brown's inventive brain. Doctor Cullen supposes it stimulant in its first operation, and Doctor Dickinson in the latitude of our author. I have always considered an increased momentum of the pulse as a sure sign of stimulant operation and increased velocity with a diminution of momentum as a sure token of the operation of debilitating powers. In a short time after the exhibition of opium, sleep is induced, the animal functions are suspended, and many strong marks of deficient excitement appear. The force of the vital functions appear to be enlarged; the pulsations of the heart and arteries are more forcible, but whether the enlarged momentum will compensate the deficient velocity is a question yet to be investigated. The short space of time that intervenes between its exhibition and sleep and other marks of deficient excitement rather manifest a sedative than a stimulant operation. Opium possesses a greater power of diminishing the irregular alternate contractions of muscular fibres than any other substance in the *materia medica*. From thence it

has been denominated an antispasmodic. Former physicians contented themselves with the knowledge of this fact without enquiring into its nature and mode of operation. Is this a specific power of opium, or are we to ascribe it to a stimulant or sedative operation? Spirits and several other stimulants possess an antispasmodic power in disorders of the alimentary canal, but I am sure that no substance will as readily take off the spasms of the remote parts of the system as opium. Great marks of debility appear after the spasms have been taken off in spasmodic disorders. Would this happen provided opium operated as a directly stimulating power? If its stimulant power was in exact proportion to antispasmodic effect, health, vigor, appetite, and cheerfulness would follow its use. But on the contrary we observe nausea, languor, fullenness and every mark of diminished excitement. This may be imputed to indirect debility—But I never can have faith enough to believe that one, two or three grains of opium, possesses so much stimulant power as in disorders of debility not only to raise the excitement to the point of health but to exceed that point, and continue the excitement until indirect debility commences. We are in possession of few if any medicines of such sovereign efficacy. In a system of principles established with such geometrical exactness as the present, we would expect in disorders of debility a sufficient quantity of opium to restore health only exhibited. The author should have established a scale of stimulant operation to complete his system.

ſyſtem. We ſhould diſtinguiſh ſubſtances from one another by their quantity of exciting power, not by taſte, ſmell or botannical relations. After the uſe of opium in fevers and in all ſpaſmodic diſorders, we are neceſſitated to have recourſe to tonic and ſtimulant medicines to complete a cure. In the aſthma and epilepſy it may ſhorten a paroxiſm but never in any quantity or exhibited for any length of time, does it produce a radical cure. Opium exhibited in the paroxiſm of an intermittent, will haſten the ſolution of the paroxiſm, but never by a ſtimulant operation prevent their recurrence. It is ſometimes exhibited a ſhort time before the expected paroxiſm, but I expect if it has a good effect it is by blunting the ſenſibility of the ſyſtem. If ſleep is a mark of diminiſhed excitement, if coſtiveneſs is a mark of languid action of the muſcular fibres, of the inteſtines, and if theſe phænomena univerſally ſucceed the adminiſtration of opium by what ſtrained inference can we make opium a ſtimulant—If a ſtimulant, why does it not invigorate the periſtaltic motion to evacuate the alimentary canal?

The hurtful effects of opium in inflammatory diſorders I expect will be urged as a proof of its ſtimulant operation. I have adminiſtered it in pleuriſies, and I muſt acknowledge with manifeſt injury. But whether the injury is to be imputed to ſtimulant operation or to a diminution of activity, may be a queſtionable point. The healthy tone of the fibres depends greatly on a repe-

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tition of contraction, and on a due quantity of nutritious chyle. And if it can be proven that opium effects neither of these ends, but on the contrary diminishes the contractile disposition of the muscular fibres, its stimulant power must immediately become doubtful. Exhibited in pleurisies it abates pain, whereas, if stimulant, we would expect as the pain depends on the diathesis, it would be increased. In pleuretic complaints, nature attempts to relieve the fullness of the system by a discharge of mucus and blood from the lungs. Exhibit the smallest quantity of opium, it instantly checks the cough and expectoration, whereas, if stimulant, it would increase the cough. The small quantity that will stop the cough and expectoration, I presume would not suddenly induce indirect debility.—Ardent spirits and other stimulants increase and aggravate both cough and pain, from which we conclude that the operation of opium is no ways analagous to general stimulant powers. A small quantity is observed to occasion wakefulness, while a large one generally brings on immediate somnolency. It is to be noted that a large quantity of any exciting power proves sedative. A small quantity of food stimulates, increases activity, while a large one overcomes the action of muscular fibres and increases deficiency of excitement. It is on this foundation that opium is generally exhibited. It suspends the contractile power of muscular fibres without raising the excitement to the point of indirect debility. It requires time and a gradual and repeated use of

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moderate ſtimuli to raiſe the excitement to 70°. And I believe it never happens from the uſe of medicines, without a gradual invigoration of the digeſtive organs, thereby enabling them to convert the food into a large quantity of chyle, and by the other functions of nature applying it to repair the waſte and increaſe the fulneſs of the ſyſtem. The tone that medicine gives to muſcular fibres, is entirely different from that ariſing from the uſe of nutrient ſubſtances. It is obſervable that people never recover their former tone and vigor until their appetite returns. And I have ſeen caſes of evident debility, where medicine (as opium for inſtance) ſeemed to blunt the ſenſibility, or, in the language of Doctor Brown, to wear out the excitability, and impede inſtead of haſtening a recovery. I do not conceive that the free uſe made of opium by the Turks to urge them on to battle, is a very powerful argument in ſupport of its ſtimulant operation. Fear is often connected with great mobility; and we will acknowledge that opium leſſens the mobility during its operation. But that it anſwers this end in the ſame manner of other ſtimuli, is queſtionable. It takes off the reſiſtance of the veſſels to the influx of their fluids, diminiſhes the velocity of circulation, retards the return of the venous blood from the head, and in this manner we are to account for the paſſive courage of the Turks in battle. The Turks do not poſſeſs the ferocious courage of the American Indians. They ſtarve themſelves to whet their courage, and they are more alert

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and active than an army of Turks under the enfeebling operation of opium. I place no reliance on its ſtimulant power, becauſe it ſometimes occaſions cheerfulneſs of mind. I have experienced the greateſt cheerfulneſs of mind, and felt the moſt firm and courageous when I have been under the operation of powers manifeſtly debilitating.

It is a univerſal property of ſtimulants to extend their influence to the remoteſt parts of the ſyſtem. Opium and ardent ſpirits, ſoon after exhibition occaſion a coldneſs of the extremeties, a mark of ſedative operation. If opium poſſeſſes a power of ſtimulating above the ordinary exciting power neceſſary to maintain health, its operation is very tranſitory and leſs durable than other ſtimuli. I will not contend that opium ſtimulates none at all, for it certainly in ſome caſes manifeſts ſtimulant operation. But whether thoſe appearances are to be aſcribed to its exciting the action of the *vis medicatrix* (agreeably to Doctor Cullen's opinion) or to a direct ſtimulant operation, I will not determine. I will obſerve however, that the phænomena of ſtimulant and ſedative operation exiſt together; their boundaries are not ſo well marked and defined as other ſtimuli, and that if ſtimulant, its operation is *ſui generis* and not analagous to any other ſubſtance. The homogeniety of operation of every ſubſtance in nature, as aſſerted by Doctor Brown, appears to me doubtful.

The aſſertion of the author that opium is the moſt

moſt powerful medicine in ſupporting the watching ſtate, is altogether incredible, and requires a degree of faith equal to the novelty and falſity of the aſſertion. There are perſons that are remarkably wakeful under the operation of opium, whether from the ſmallneſs of the doſe, or an idyoſncracy of body, is not always very apparent. The common effect of opium is to induce ſleep. It is exhibited with that view by every phyſician ſince its firſt introduction into the materia medica. They could not have been miſtaken in a fact of ſuch univerſal notoriety. Doctor Brown might as well attempt to reaſon phyſicians out of their ſenſes. The operation of opium, like every other ſubſtance, is various, depending on the condition of the ſyſtem at the time of exhibition. This was known to the faculty, as well as that opium increaſes ſome pains, before the appearance of the new theory. Cathartics the moſt violent in their operation, are ſuddenly checked by the adminiſtration of opium. And as cathartics, although their evacuation occaſions debility, yet as their mode of effecting evacuation is ſtimulant, we would ſuppoſe that opium inſtead of interrupting would increaſe the evacuation of the bowels. We hold it as a medical truth, that all ſtimulants operate by increaſing the contraction of muſcular fibres. Opium therefore as a ſtimulant, ſhould increaſe the contraction and activity of the inteſtines and haſten the expulſion of their contents. But as it evidently has a directly oppoſite effect, we preſume that the operation of opium in this inſtance is not ſtimulant.

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In a note upon Paragraph CCXXXII, the author says that *excitability is one uniform, undivided property over all the system.*

That a certain portion of excitability is assigned to every part of the living system our author acknowledges, and also that a greater quantity may be assigned to one part than another of equal nervous importance. After this explicit acknowledgement of the different degrees of excitability in different parts, with what propriety can he infer its uniformity? The word *uniform* carries an idea of equality in quantity in every part of the system. The property of indivisibility ascribed to the excitability by the author, is to be doubted. For as the excitability is inherent in *medullary matter*, and as when a limb or member is taken off, some portion of medullary matter must also be taken off, the divisibility of the excitability therefore follows as an irresistible conclusion.

The excitability in its nature, like immaterial beings is indivisible. But in effect if it is inherent in medullary matter, it is often divided, as in gangrenes, loss of limbs, &c. If the excitability was generally and uniformly diffused over the system and had a ready instantaneous communication, why would not a medicine in contact with any one part, extend its effects to every other part, and in case of disease a complete cure be established. But the excitability is not uniform. A wound in the root of a vegetable

getable, or in the head of an animal, is followed with worſe conſequences than a wound of equal extent inflicted in any other part. In the mind the excitability is ſubject to alternate periodical increaſe and abatement, while in other parts it for the moſt part remains ſtationary.

Paragraph CCCV. *The cure of any ſthenic diſeaſe is improperly truſted to bleeding alone, becauſe that although that operation may reduce the excitability in the greater blood veſſels, perhaps too much; yet in the extremities of theſe as well as in the reſt of the body it is not ſufficiently reduced.*

The operation of exciting and debilitating powers I have ſuppoſed to be very general over the ſyſtem. Even the partial application of exciting or debilitating powers I expect their effect in the part is readily communicated to every other part of the ſyſtem. The numerous anaſtamoſes of veſſels; their general extenſion and communication; together with the connection of the arterial and nervous ſyſtems is ſuch that the excitement cannot be taken off in the larger veſſels without immediately extending that effect throughout the animal œconomy The operation of blood-letting has a ſudden and univerſal effect. It is not confined to one part. The reaſoning of the author does not appear like contemplating the body as a whole. I conſider venœſection a powerful mean of reducing ſthenic diatheſis, becauſe *it evacuates a ſtimulus as*

much more powerful than any other, as it is directly applied to a greater extent over the ſyſtem. The veſſels of the ſtomach, the perſpiratory veſſels, and all the ſecretory veſſels participate in the effect of venœſection and cathartics. The exhibition of an emetic after venœſection by the conſent of the ſtomach and ſkin, may effect a diaphoreſis, and that a diminution of excitement greater than any other medicine.

The good effects of vomiting do not conſiſt ſo much in unloading the mucous follicles of the ſtomach and inteſtines as in removing the ſpaſm of the extreme veſſels. If medicines operating on the ſtomach did not extend their influence over the ſyſtem, cleanſing the ſtomach would have no more effect in reſtoring the healthy ſtate, than cleaning one pump-box would reſtore the uſe of a pump when both are foul. But we are more wiſely organiſed; all the functions depend on the vital principle; every article of diet is ſubdued by the vital principle and made ſubſervient to life, and every medicine acts on the vital principle, and as *it* is generally diffuſed, the operation of medicine is general and univerſal. We do not mean to ſhew in what the vital principle conſiſts. We aſſert that it exerts its influence from within outwards, as it appears that the blood in all animals moves from the heart to the extreme parts.

The operation or power in the vital principle ſeems to be weaker in the extreme parts than the internal parts. A coldneſs of the extremities is

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the first symptom of every general disorder. The exciting hurtful powers operate on the extreme sensible parts. They produce spasm, and spasm an inverted peristaltic motion of the arteries. This opinion is rendered probable by the rolling wiry feel of the arteries in every general disorder, and especially evident in disorders of great obstinacy.

To return from this digression to the necessity of cathartics and emetics in sthenic diathesis, we assert, that on the principle of this system, they do not appear indispensably necessary. Immediately after venœsection, a contraction of the vessels and muscular fibres takes place, so that if any sordes or foulnesses loiter in the emunctories, they will be squeezed out by the efforts of contraction. The excitement being equally reduced in the muscular fibres of the intestines, they of consequence must contract and expel their contents. It is to be observed that an increased or diminished excitement retards the discharge of the fœces, so that to hasten its expulsion, diminished excitement must be raised, and increased excitement diminished.

I acknowledge these observations would not be considered very orthodox on any other principle except our author's. But they are perfectly consonant to his observations on the effects of stimulants in colic and worms. If there be no necessity of administering medicines to operate on particular functions in those disorders, I am sure

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it would be equally frivolous in general ſthenic diatheſis.

Paragraph CCXXXII. *While the general affection for the moſt part precedes the local affection and never ſucceeds to it.*

The exanthemata and peripneumony, although they are equally diſorders of ſthenic diatheſis, yet the local affection of the one is entirely different from that of the other. It is certainly neceſſary that the fever ſhould precede the eruption in exanthematic diſorders, but in peripneumony the caſe is widely different. The general muſt either precede, accompany, or ſucceed to the local affection. Therefore I cannot ſee the propriety in ſaying that the general for the moſt part precedes, and never ſucceeds to the local affection. If it never ſucceeds it muſt always precede, for their coincidence is a rare and accidental (if poſſible) occurrence. Doctor Brown holds out an idea that the diatheſis and the diſorder are one and the ſame. A ſthenic diatheſis may exiſt without diſturbance in the ſyſtem, and I have ſeen caſes of pneumonic inflammation ſucceed to diſorders of debility and happen in perſons manifeſtly of a weak aſthenic habit of body. I therefore conceive that the diatheſis and the diſorder are not exactly the ſame, and that they often ſtand in the relation of cauſe and effect. The ſymptoms conſtitute the diſorder. The ſymptoms flow from the diatheſis. Fever, a pain in ſome part of the thorax, cough and difficult reſpiration

ſpiration are the ſymptoms of pneumonic inflammation. It is a matter of little importance in the treatment, whether the pain precedes or ſucceeds to the fever. To give a faithful and true explanation of the production of local inflammation would be a difficult taſk. But I lay it down as an axiom, that an inequality of excitement or nervous influence, muſt exiſt previous to the production of local affection. The reſiſting energy of the veſſels of the part muſt be weakened, or the reſiſting energy of all the other veſſels of the ſyſtem enlarged. One or the other muſt happen, or we can have no poſſible conception of the production of local affection. Which of theſe events is the moſt likely to happen? Or if one happens, does the other follow of courſe? The fever in pneumonic inflammation ſcarce ever obſerves thoſe exacerbations and remiſſions that happen in genuine idiopathic fevers. This ſtrengthens the opinion that the fever is the conſequence of the local inflammation, not original but ſymptomatic. Although the local affection ſometimes ſupervenes to general diſorders, I have obſerved that pain exiſted generally prior to much diſturbance in the vital functions.

Paragraph CCCXXXVI. *The ſhivering and ſenſe of cold depend for their cauſe upon the dryneſs of the ſkin. The languor and laſſitude point out a higher degree of excitement in the brain and muſcles, than can be conveniently borne by the excitability confined within certain boundaries.*

Why

Why have recourse to one symptom to explain another? The cause of all the symptoms may be sought for in the general system. If either the one or the other are anterior in point of time I should presume that a shivering and coldness made their appearance first, and the dryness of the skin was the consequence of the former. Shivering and coldness are symptomatic of the formation of spasm, and the dryness of the skin a consequence of spasm; so that although all the symtoms are imputable to the diathesis, yet the shivering and coldness and dryness of skin, appear to stand in the relation of cause and effect.

In paragraph CLIV, he says *the dryness of the skin admits the same explanation as the shivering and sense of cold.*

From what data does our author infer a higher degree of excitement in the brain and fibres of the muscles? The sense of languor that precedes disorders, originates from a diminished or increased energy of the whole system. The author boasts of the uniformity and simplicity of his principles. This increased energy of the brain does not square with his principles. If there exists in sthenic disorders a greater proportional excitement in the brain than any other part, why may not a less proportional energy of the brain and muscles exist in asthenic disorders? And what would this be but an admission of Cullen's theory? The author has taken great pains

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to avoid the imputation of imitation, by adding the muſcles to the brain. Cullen ſuppoſed the brain the origin of the nervous ſyſtem, and the nerves diſtributed in the extreme veſſels to be the principal ſeat of the operation of ſedative and ſtimulant powers, and by a peculiar ſympathy with the brain, any alteration in the ſtate of the former, would be very readily communicated to that of the latter. But this reaſoning does not apply to the ſyſtem of Doctor Brown. What poſſible conſtruction can be put upon the words "*conveniently borne by the excitability confined within certain boundaries.*" We are all ſenſible that neither the excitement nor excitability are infinite. They are confined within certain boundaries. We alſo know that the excitability may be raiſed or diminiſhed. But why the excitability cannot bear the excitement raiſed without conſequent ſuffering, the author has not informed us.

Paragraph CCCXXXVII. *The dryneſs of the ſkin is occaſioned by the great excitement and denſity of the fibres that encircle the extreme veſſels, diminiſhing their diameters to ſuch a degree that the imperceptible vapour cannot be taken into them, or if taken in, cannot be tranſmitted.*

What fibres are thoſe that encircle the extreme veſſels? The muſcles are longitudinal inſtead of circular. The ſkin cannot embrace the extreme veſſels more cloſely in the morbid than healthy ſtate, except a ſpaſm is formed. Admit for a moment

moment that the extreme veſſels were ſurrounded with circular elaſtic fibres, what proof can we advance that the excitement and their denſity is increaſed? Their denſity muſt be increaſed either as ſimple or living ſolids. If ſimple, there muſt be a greater coheſion of particles, and if ſo why not aſſign the cauſes of its production. If increaſed as living ſolids, it muſt be by a forcible and durable contraction, which is ſynonymous with ſpaſm. But in a ſucceeding ſentence—"*It is not ſpaſm, it is not conſtriction from cold, but a ſthenic diatheſis ſomewhat greater upon the ſurface than any other part.*"

If the excitement is greater on the ſurface in ſthenic diſorders, why may it not be leſs in aſthenic diſorders than any other part? If the excitement is greater on the ſurface than any other part, it muſt originate from the greater ſenſibility of the ſurface, or a direct application of the exciting powers. As the ſtate of increaſed excitement is the ſame with increaſed tone, and as increaſed tone is always accompanied with increaſed contraction, and contraction is ſynonymous with ſpaſm, what is the ſpecific difference between his theory and Doctor Cullen's.

In a following ſentence he ſays, that the ſtimulant energy of heat ſucceeding cold, is a powerful exciting cauſe of ſthenic diſeaſes. The ſpring is the ſeaſon moſt ſubject to inflammatory diſeaſes. But I do not conceive the warmth of ſpring produces ſthenic diatheſis: for the effect

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of any conſiderable degree of heat is to debilitate. The cold of winter braces and increaſes the tone of muſcular fibres, and the irregular degrees of heat ſucceeding gives occaſion to the appearance of inflammatory diſorders. Their appearance is ſometimes from partial application of heat. But in no inſtance can the heat be conſidered as the efficient cauſe. The heat is the occaſional cauſe. Heat does not ſeem to produce diatheſis, but to convert diatheſis into diſeaſe.

Paragraph CCCLXI. What does the author mean by an inflammatory or catarrhal affection of ſome of the large joints accompanying a phrenitis? It is a novel and ſingular mode of ſpeaking to apply the epithet *catarrhal* to affections of the joints. In a ſucceeding paragraph he ſays, an inflammation in its proper form does not exiſt in this diſorder; but an approach to inflammatory ſtate exiſts in the joints, in the muſcles, and eſpecially over the ſpine or about the cheſt, or in the bottom of the throat, or there is a catarrhal ſtate.

From what ſource were theſe new ſymptoms and affections drawn. Not ſurely from the bedſide of the ſick. What is the proper form of inflammation? He admits that venœſection gives great relief; and yet he ſays there is no inflammation, but a ſtate particularly over the ſpine approaching to inflammation. From what does he infer that a ſtate approaching to inflammation, exiſts over the ſpine in preference to every other.

part of the ſyſtem. The ſame general remedies that are employed in other inflammations, are employed in this diſeaſe, and ſometimes with the ſame ſalutary effect.

Paragraph CCCLXVIII. *Contagion ferments without any change of ſolids or fluids, it fills all the veſſels and then is gradually ejected by the pores.*

Is it not unaccountable that a ſtate of fermentation ſhould exiſt without inducing any change in ſolids or fluids, contrary to its known effects in every ſubſtance which is ſubject to fermentation. What exempts the animal œconomy from this general law? The vital principle may modify the operation of any fermenting ſubſtance. But if it prevented any change taking place it muſt be by ſubduing the contagion in its firſt entrance into the body and the diſorder would not exiſt. If no change is effected, what probable cauſe can be aſſigned for the moſt of eruptive diſorders never occurring but once in the ſame perſon. This is a good ground for diſcriminating the exanthemata from the other phlegmaſia.—They differ in their eruptive appearance, in their cauſe, although a ſimilar method of cure anſwers for both in the ſame diatheſis, I yet think it a good foundation of diſtinction. The other phlegmaſiæ occur annually, and the exanthemata very ſeldom oftener than once in an age. The ſtimulant operation of contagion I doubt. 1ſt. Becauſe eruptive diſorders are often attended with manifeſt marks of debility and putrefaction

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in their first appearance. This could not happen unless the force of stimulant operation was so great as to produce indirect debility. And this is improbable. 1st. Because contagious disorders generally appear in states of the atmosphere unfavorable to produce a sthenic effect. 2nd. That contagious disorders more commonly affect the weak and valetudinary than the strong and robust. Whereas, if their operation was indirectly debilitating, the strong and robust would be most subject to contagious disorders.

Contagions in general do not possess a stimulus sufficient to maintain the healthy state. Contagion is a matter foreign to the body, not necessary to support life, and from its evident effects on the body, we judge it to be often of a sedative nature. That catarrh uniformly succeeds heat as an efficient cause, as asserted in paragraph 411, I am very much disposed to doubt. It often succeeds heat I will allow, but in so far as it is a sthenic disease, I consider it as the offspring of cold. It is not always a sthenic disease, for it often occurs in weak debilitated habits. The alternation of heat with cold is apt to produce sthenic diathesis. But I am confident an equal, steady and uniform application of heat would never produce a catarrh. The affection of the fauces is a solid argument in support of an unequal degree of heat or cold having been applied. That it is not the sole effect of heat or cold is proven from its occurrence both in the spring and fall seasons. The good effect of sweating

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in sthenic disorders I have no experience. From the testimony of others whose experience I have no reason to question, I believe it seldom contributes very considerably to their solution. Doctor Cullen says that sweating is seldom if ever serviceable in Peripneumony and as far as my experience goes, it exactly coincides with this opinion. I have observed critical sweats produce an entire solution of pneumonic inflammation. I have also observed that any considerable increase of perspiration was difficult to be raised, whether owing to a spasmodic state of the extreme vessels, or to an excessive excitement existing on the surface I will not decide. The means he employs to raise a sweat are of a stimulant nature. He advises laudanum and Dover's powders, together with a considerable covering of bed cloathes, means agreeably to his own principles, manifestly stimulant, and as such must serve to increase instead of diminishing the sum total of excitement. The balance between the different excretions of the system is not folly, the cure of many disorders depend upon their equilibrium. That such a balance exists the experiments of Sanctorious prove. But it is not so exact but occasional deviations may exist without impeding health.

No considerable diminution or increase can exist without disease being the inevitable consequence. It has long been a noted maxim that an increase of one evacuation is always followed by the diminution of another. Whenever a

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cure of any disorder depends upon exciting any evacuation, the increase of another will certainly diminish that upon which health or a cure depends. Any considerable Catharsis in fevers is always accompanied with a dryness of the skin. A diarrhœa although an asthenic disorder, is often relieved by a determination of fluids towards the surface. If in inflammatory disorders a greater proportional excitement exists in the part affected than in other parts of the system, and if the fluids contained in the vessels stimulate; will, determining a greater quantity of fluids into them diminish their excitement. He acknowledges that there is an objection to sweating, because the means employed are generally of a heating stimulant nature. In a former page he asserted, that the application of cold was the most powerful means to diminish the excitement, and further that cold increased perspiration. From this concession the good effects of sweating is not very conspicuous on his own principles.

The cause of menstruation both in its first appearance, continuance and final cessation, our author imputes to a particular confirmation of the uterine vessels and the stimulus of love. That a peculiar conformation of the uterine vessels different from all other red vessels exists I have no doubt. And that nature designed their evolution to be effected at a particular age. But that the stimulus of love contributes to the first appearance of menstruation, I deny. It may serve to keep up the the discharge after it is established. The passion of love does not exist antecedent to the discharge.

Love

Love seems to be the consequence of it. The genital system is almost independent of the general habit. It is true stimulating food, spiritous liquors, &c. excite the passion of love by an operation on the general system. The energy of excitement is not much greater in the general system at the time of menstruation than at any other period. The energy of the uterine vessels is no doubt enlarged. The author has thrown no new light upon the subject of menstruation. Barely imputing it to excitement, is a very miserable account of its production. The *why* and *wherefore* are not accurately ascertained. Does a partial or general plethora exist at the menstruating periods? What particular conformation of female œconomy gives occasion to the existence of plethora? The cause of love and menstruation are one and the same—they continue an equal length of time and then disappear. It becomes the author to ascertain upon mathematical principles why a handsome man will at that particular period excite the passion of love which the same person could not have excited a year before, and which the handsomest female never excites. A deficiency of menstruation may undoubtedly be the consequence of high sthenic diathesis in the uterine vessels as well as the general system. I can see no incompatibility in deficient menstruation originating from this state. It is no ways similar to any artificial discharge, therefore arguments drawn from them cannot apply to the menstrual discharge. The morbid states of this discharge are commonly to be ascribed to debility,

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and I have no doubt a ſpaſmodic ſtate of the uterine veſſels ſometimes exiſts. The cauſe of an obſtructed menſtruation is often a want of enjoying the pleaſures of love ; but its operation is on the mind weakening the whole ſyſtem, and of conſequence the energy of the uterine veſſels. It is not a very late diſcovery that ſtimulants and nouriſhing diet remove the morbid ſtates of this diſcharge. It has been a mode of cure in general uſe for many years. If he claims any merit in its introduction, it is an arrogant aſſumption. The ſcurvy has both by ancient and modern phyſicians, been imputed to ſalted and corrupted meat ; unfermented farinacea, want of exerciſe and almoſt every debilitating cauſe enumerated by Doctor Brown. Grief for the loſs of liberty and friends, a diſlike to their preſent employment, the awe which the ſeverity of diſcipline keeps them in, are circumſtances which have very little effect on the minds of ſailors—they are commonly the moſt careleſs, eaſy and contented, of any deſcription of men ; they generally evidence more uneaſineſs on land than when at ſea, and ſo far from having an averſion to their mode of life, they are uncommonly attached to it. The fear of battle has (if any) a very momentary influence with ſailors. Doctor Brown is but little acquainted with the nautical character or he is determined to introduce a few new remote cauſes at the expence of truth. I cannot conceive what the author means by this diſorder ſtanding on a narrow foundation in former theories. It has never in any theory been aſcribed to the ſingle operation

operation of one cauſe, but to the aggregate operation of a variety. The proximate cauſe of ſcurvy in the theories of the preſent day, is ſaid to be an acrimony or vitiation of the fluids. A correſpondent debility of the ſolids is unqueſtionable. Some of the cauſes of ſcurvy ſeem to operate immediately on the ſolids and ſome on the fluids. A difference in aliment will undoubtedly alter the quality, mixture and conſiſtence of the fluids. A perſon living altogether on animal food will have a difference in quantity and quality of fluids from a perſon living entirely on vegetable diet. The maſs of blood made from animal food, poſſeſſes a greater ſtimulant power than blood made from vegetable diet. It is more quickly animalized, and runs more ſpeedily into a ſtate of putrefaction. From theſe conſiderations, I think the humoral pathology is retained with more propriety in this, than any other diſorder. A further ſupport of this opinion is, that the ſcurvy ſeldom appears, except in conſequence of long continued uſe of ſalted proviſions, and an entire deprivation of vegetable aliment. Vegetables uſed plentifully have a powerful effect an reſtoring the healthy ſtate. I have no doubt that a return to land exerciſes, to a pure dry air, and to a healthy mode of living, will effect a cure. But whether it is an attachment to my early medical principles, or from reaſoning, (for I have little experience in the ſcurvy) I am diſpoſed to believe from the teſtimony of others whoſe veracity I have no reaſon to queſtion, that the plentiful uſe of acids and acefcent vegetables greatly

expedites

expedites the cure. The motion of the veſſel tends to induce a coſtive habit, and that circumſtance may be ſuppoſed to have conſiderable influence in producing the diſorder, and in retarding its cure. It is an opinion of mine, that heating cathartics are not ſo ſalutary as cooling aperient medicines. This opinion, if true, corroborates the notion of a diſſolution of the fluids being the proximate cauſe of the diſorder. Coſtiveneſs by hindering the diſcharge of the bile, pancreatic juice and other fluids into the inteſtines, occaſions their abſorption, and hence the degeneracy and acrimony of the maſs of blood.

The gout is a diſorder whoſe proximate cauſe is not well ſettled. Its appearances are ſo vague and anomalous, that a ſatisfactory pathology is difficult to eſtabliſh. It is, however, confirmed by the experience of ages, that it is the legitimate offspring of luxury and intemperance. Ebriety and gluttony have uniformly been its antecedents. The notion of its being hereditary, our author ſays is a tale, or the fundamental part of his doctrine will fail. If he was willing to put the truth of his doctrine upon this iſſue, I would have no objection. But the ſuperior utility of his principles may more handſomely be combatted on the grounds of merit. The faculty by their own experience may determine whether they have judged right and his doctrine fail, or they have been deceived and his doctrine be eſtabliſhed. I will obſerve by the by, that I can conceive no incompatibility or incongruity be-

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tween the gout's being an hereditary diſeaſe and the fundamental part of his doctrine. He acknowledges that a certain texture of the fibres or ſtamina is more favorable to the approach of certain diſeaſes than the oppoſite form, and as the gout belongs to one form or the other, and as I expect he will grant that parents generally tranſmit to their offspring a conſtitution ſimilar to their own, I can conceive no abſurdity in denominating it an hereditary diſeaſe. The argument he employs to refute this opinion, I conceive not to be well adapted to this purpoſe. Does it prove the diſorder not hereditary becauſe Peter by adapting his excitement to his ſtamina may evade the gout of his father Paul? Surely not—For it can be as mathematically proven as the truth of his doctrine that deſcendents of Podagrics, cannot bear with impunity the effect of the ſame exciting powers as others. An equal exceſs of drinking or eating will have a greater tendency to fetch on a paroxiſm of the gout in them, than in a perſon not poſſeſſing the ſame hereditary taint. It has been admitted by Doctor Cullen and others, that the ſon by temperance and exerciſe that is ſuiting his excitement to his ſtamina, may evade the gout of his father. Could the medical world be ſo long deceived. The medical world has been groping in a long night of darkneſs—and happy is it for the human race, that this great luminary has ariſen to brighten the darkened hemiſphere of medicine. I can obſerve nothing in the opinion fundamentally incompatible with genuine medical principles, and

as the weight and number of a teſtimony is in favor of its being hereditary, I ſhall believe it until the contrary is proven. The reſemblance between the gout and dyſpepſia is not very apparent. It is true that dyſpepſia reſembles the gout of the ſtomach; but the diſtinguiſhing mark of the gout is its inflammatory appearance in the extremities. It is not a trifling immaterial circumſtance—It is the criterion of the diſorder. The gout has been connected with the gravel.

With reſpect to the cure of this afflicting complaint, I muſt obſerve that although evacuations would be extremely improper when the diſorder has frequently recurred and marks of inanition exiſt, yet I conceive there may be inſtances in the beginning, in which venœſection and other evacuations would be admiſſible. A plethora may exiſt when there is great deficiency of vigor in the living ſolids. The firſt full meal, if properly digeſted and aſſimilated, will no doubt conduce to fill the arterial & venous ſyſtem, and every ſucceeding indulgence in eating and drinking will ſerve to increaſe that effect until repeated indulgence carries the excitement up to 70°, at which point indirect debility takes place. And as this is an inſtantaneous change of condition in the ſyſtem, the former fullneſs of the veſſels muſt continue, without ſome evacuation is made. In ſuch a condition of the ſyſtem a ſmall evacuation relieves from oppreſſion; the fibres contract, and the tone of the ſyſtem is in ſome meaſure reſtored. It is even admiſſible in ſome caſes of gangrene,

grene. A gradual reduction of high living will gradually produce the ſame effect.

Our author ſays, to prolong the intervals of health and prevent a recurrence of the paroxiſms the remedies are all the reverſe of the hurtful powers. The hurtful powers are full living, eating and drinking luxuriouſly. But the preſcription is to take rich food plentifully, with the reſtriction of keeping within the ſtimulant range. If the diſorder is not the effect of one full meal, as it moſt certainly is not, full living muſt be the hurtful power that induced the diſorder, conſequently ſhould be refrained from. It will certainly take a larger quantity of food or exciting powers to raiſe the languid excitement when the ſyſtem is oppreſſed with indirect debility, when the excitability is worn out, than before the approach of the diſorder. Therefore, to keep your medicines within the ſtimulant range, you muſt increaſe the ſum total of ſtimulant or hurtful powers. In a former part of this work we are directed in indirect debility, to deſiſt from the uſe of ſtimuli gradually, to allow the waſted excitability to accumulate. Why a different preſcription in the gout? Is it becauſe our author cannot brook imitation? This would be to tread in the footſteps of his predeceſſors. Few have adviſed evacuations in this diſorder. They have alſo bore teſtimony againſt a ſudden tranſition from luxurious living to a very meagre ſcanty diet. Inſtead of recommending full living, plenty of animal food, and a ſparing uſe of vegetable aliment

ment, would it not have been more consistent with his theory and common sense, to have advised a more and more sparing use of animal food, exercise and a proper regulation of the non-naturals. This has been the gouty regimen from the earliest ages of physic, and constant experience has established its beneficial consequences. A thorough cure is not to be expected from medicines. All the diffusive stimuli on earth only serve to increase the disorder. Is it any wonder our author fell a victim to his own theory? Hard thinking to compose such a wonderful system of physic; hard drinking and luxurious eating must land every podagric in the world to come. A gradual reduction of the hurtful powers, is a rational consistent mode of cure. Accustoming the system to less and less of stimulus, the excitability will accumulate, the natural excitement will be restored, and the body be free from disorder. But a contrary plan will continue the gout, the opprobrium medicorum. What necessity for the distinction of the gout of stronger and weakened persons? Or why any necessity for constituting a mild and violent disorder? They differ only in the degree of excitement. They require the same medicines, and therefore the distinction is frivolous and unmeaning. This is not preserving unity and simplicity in theory.

Paragraph DCXCV. *In the cure of anarsaca we must invigorate the parts where the atony and laxity prevails, as in the skin.*

What

What evidence have we of a greater atony in the skin than any other part. The anarsaca arises from atony of the exhalants that empty into the adipose membrane, and the absorbents which return the lymph into the mass of blood. This is surely an internal *local affection*, unless he means to confine the meaning of the phrase to abdominal or thoracic affections. The medicine he enjoins, stimulant food and peruvian bark, certainly have no peculiar or pre-eminent operation on the skin. He has in a former part of this work defyed the world to find a medicine that possessed a specific power of operating on a single part. Medicines too are to be directed to the *excitement*, not to the *fluids or solids*, and the excitement is one uniform undivided property over all the system. This local indication comes a little aukward from Doctor Brown.

Paragraph DCXXXI. *Besides the general indication of cure for asthenia, that suited to this case (meaning the general dropsy) must be particularly directed to the whole vascular system, and especially about their terminations and the commencements of the absorbent veins.*

The remedies (he says) are the usual ones.—Do the usual stimulant remedies operate specifically on the terminations of the vascular system and the commencement of the absorbent veins? Certainly not. They invigorate the whole system, and if any one part labours under greater atony than another, we must raise the excite-

ment

ment in the general ſyſtem above the point of health, to raiſe the excitement of the local affection to the healthy ſtate. Our attention in moſt caſes ſhould be directed to keep up an equality in the excretions, and a uniform exertion of the vital principle. The means to effect this end are very ſeldom your violent diffuſible ſtimuli. But his medicines operate as it were by magic. We would conjecture that he poſſeſſed a knowledge of legerdemain, by the ſurpriſing inſtantaneous cures he performs with the diffuſible ſtimuli. The application of heat can by no means be ſalutary, unleſs ſtrong enough to excite a profuſe perſpiration, and even in that caſe the debilitating effects of heat would more than counterbalance the good effects of the diſcharge. Even frictions debilitate, unleſs the collection of water is ſmall, or ſome evacuation made ſo as to give the fibres a power to contract. Every effort of muſcular fibres to contract, unleſs ſucceſsful, is followed with an encreaſe of debility. Hence reſults the propriety of ordering frictions of the legs in the morning only when the intumeſcence has ſubſided. The ſucceſs of all ſtimulating means, muſt be the ſame, unleſs the complaint is incipient and very ſlight. An inſtance of anaſarca with aſcites, lately fell under my obſervation in which evacuant, and I believe tonic remedies, had been tried without any ſucceſs. I adviſed Doctor Bond's pills *for the dropſy*— They were exhibited by the attending phyſician without any conſiderable diſcharge of water until punctures were made in the feet. The diſcharge by

by the feet, although inconsiderable compared to the great quantity collected in the abdomen and adipose membrane, made such an alteration in the system, that by continuing the pills a fortnight, an entire evacuation of the water was effected. The evacuation by the feet relieved the distention. The muscular fibres contracted with facility. Muscular contraction is the mode in which medicines operate. And if the system is so distended as to prevent contraction, their operation will be similar to the operation of medicines in a cadaver.

Apoplexy and palsy are disorders of indirect debility. They may sometimes be accompanied with sthenic diathesis. They seldom if ever arise from directly debilitating powers. Extreme cold is said to produce a palsy of the members. Opium and many poisons produce an apoplexy, which has been denominated *apoplexia venenata*. Whether the operation of opium in producing this disorder, is directly or indirectly debilitating I will not determine. But I will observe that the operation of opium exhibits phœnomena very much resembling the apoplexy. The pulse is slow, its energy diminished, a disposition to sleep, and a considerable want of sensibility. These symptoms appear so soon after the exhibition of opium, that we can hardly infer an indirectly debilitating operation. The good effects of opium in diminishing the deficiency of feeling on the surface, I doubt. Indeed opium possesses no specific power over the skin in preference to

other

other parts. These are disorders of the nervous system, not cutaneous affections. I have known opium occasion an itching of the nose and skin, but it is such a rare occurrence that I impute it to idiosyncrasy. Evacuations are sometimes adviseable on the same grounds. I have proven their propriety in the gout. But to procure as large evacuations as in peripneumony must be prejudicial. The same rule in the use of stimulants should be observed, as in the gout. They should gradually be left off. Let nature accommodate herself to a healthy quantity of stimulus.

From paragraph DCLVI, to DCLXVIII, the reader may see the author's theory of fevers.

That the phænomena of fevers in all their variety of types can be satisfactorily accounted for upon the principle of simple debility, is not a matter of doubt only, but positive falsehood. We will suppose that all the remote causes of fever, such as cold with moisture, heat with dryness, and scantiness of food, were to exert their united operation upon the sentient principle of life, and occasion a deficient excitement of the extreme terminations of the arteries, what would be the natural effect. A languor, lassitude and paleness would succeed, but no actual fever would be induced. It is impossible to account for the phænomena of fever, without having recourse to spasm and the *vis medicatrix*. The remote causes of fever occasion debility, but Doctor Cullen says it is not obvious how debility

occasions spasm.* He, therefore solves the difficulty by imputing the spasm and cold stage to the operation of the *vis medicatrix*. The sudden transition from an apparent state of health to the cold stage of fevers, the stoppage of all the excretions, the shrinking of all the external parts, are indubitable proofs of a formation of spasm on the extreme arteries. Although, as Doctor Cullen says, it may be imputed to the weaker action of the heart and arteries, yet as these symptoms continue after the action of the heart and arteries seems to be restored, there is reason to believe a spasmodic constriction has taken place.§ To give a mechanical and satisfactory account how the spasm is formed, would be a difficult, if not an impossible task. In this difficulty we are obliged to have recourse to the *vis medicatrix*. To raise a counteracting power, against the heart and arteries, would at first view appear to be a hurtful instead of a salutary power. But when we reflect that debility alone would never excite the action of the heart and arteries to restore the tone of the extreme vessels, the necessity of the spasm appears obvious. Spasm acts as an indirect stimulus to the sanguiferous system, thereby exciting the action of the heart and arteries to restore the tone. This will account for all the phænomena of fevers, and no other theory so satisfactorily. It appears to be a circuitous mode for nature to do her business in: but as the poet says, in human affairs—"a thousand

* *Vide Cullen's First Lines, page* 38.

§ *See Cullen's First Lines, page* 36.

ſand movements ſcarce one purpoſe gain"—So in the operations of nature, it often takes two ſteps to gain one purpoſe. If debility was the only conſideration to be contemplated as the proximate cauſe of fever, why do not the ſymptoms of fever appear in a more gradual manner? The firſt onſet of fever is from an apparent ſtate of health to coldneſs, ſhivering, pain in the head and bones, often delirium, and other ſymptoms of ſpaſmodic conſtriction. Unleſs the author can prove debility to be inſtantaneous, it will by no means account for the phænomena of fevers. I have long thought there was ſomething in fevers that had evaded the reſearch of phyſicians, and every day's experience confirms me in that belief. Whether it is an unequal balance of the different parts of the ſyſtem, or an unequal ſpaſmodic conſtriction, or unequal debility, or the obſtinacy of the two latter, I will not, cannot determine. But ſuch is the fact, that after all the ingenious theories of fevers that have been publiſhed, ſtill we often meet with caſes that baffle the ſkill of the moſt experienced. I am convinced that if a fever can be reduced to ſimple, pure debility, the equality of the excretions reſtored, and the functions no way impaired but in vigor only, that we have it in our power by ſtimuli to reſtore the healthy ſtate. It has for many years paſt been firmly believed both in this country and in Europe, that marſh effluvia produced intermittent and remittent fevers—and Doctor Cullen infers from the apparent debility in fevers, that they are of a ſedative nature,

ture. Our author says he has proven that marsh effluvia are not the only cause of fevers. We admit it. But they are oftener the cause of fevers than he endeavors to make the public believe. He refers to paragraph DCLIII, IV, V, in which he treats of a tetanus, but I see no refutation of marsh effluvia being the remote cause of fevers. In consequence of this futile and imaginary refutation of marsh effluvia being the remote cause of fevers, he says that the change of excitement alone is the universal source of all general diseases. Would the author stifle inquiry and not advance a step in the investigation of causes. Marsh effluvia certainly exert a hurtful operation on the living system. Fevers occur most frequently in times and seasons most favorable to their generation. Dry seasons, when the waters become stagnant and the heat exhales a great quantity of vapour in the air, are the seasons of intermittent and remittent fevers. Other causes, as cold with moisture, a spare, scanty diet and excess in venery by their debilitating operation often contribute to produce the same effect. But as they are oftener and most generally the result of these noxious agents, they have been the peculiar objects of physicians' attention in tracing the remote cause of fevers. No one asserts that every different type of fever is to be referred or attributed to specifically different effluvia. The difference of type may be imputed to difference of constitution, to the different conditions of the same body, to the continuance of the disorder & debilitating powers; for we often see a

quotidian

quotidian change to a tertian, and a tertian to a quartan. The difference of type may more properly be attributed to the greater or leſſer operation of the effluvia, than to any ſpecific change in its nature. It ſhould appear that, if the cauſe of fever was debility, the leſs frequent the paroxiſms the leſs debility, and e contra; and it would ſeem from the ſame principle, that a leſs quantity of medicine would cure a quartan than quotidian. But this is not true. Some conjectures may perhaps obviate this difficulty.

1ſt. Although the debility in a quartan may not be as great as in a quotidian, the failure of the cortex and other medicines may be imputed to congeſtions of blood, or obſtructions in the liver ſpleen or ſome of the viſcera. The author may ſay that medicines altering the ſtate of excitement would remove the local affection. But I am confident that ſtimulants often fail when the excitement is raiſed or lowered conſiderably, higher in one part than the general ſyſtem, or in common medical language, when a local affection exiſts in a part.

2d. The ſpaſm of the extreme veſſels may not be ſo completely relaxed as in the paroxiſms of other types. Theſe or ſome other circumſtances of the ſyſtem fruſtrate the operation of medicines, by which it would appear, that a quartan is a diſorder of greater debility than either of the other types. But I am ſure the failure of medicine cannot be imputed to greater debility,

debility, for this reaſon, that a moderate quantity of the cortex will moſt commonly effect a cure of the quotidian and tertian types, when ten times the quantity will fail of a cure in the quartan. This I impute to ſome partial or general condition of the body not yet taken notice of or explained. A circumſtance not yet obſerved is, that I have never ſeen the efficacy of the bark anywiſe conſiderable where the patient had a vigorous appetite. And as I have generally obſerved this to be the caſe in quartans (or very often ſo) we may infer that a certain condition of the ſtomach retards or impairs the operation of the cortex. Whether this ſtate of the ſtomach conſiſts in a redundant or ſcanty ſecretion of the gaſtric acid, or a greater proportional vigor in the muſcular fibres of the ſtomach than any other part, I will not take upon me to determine.

The next abſurdity of our author in his pathology of intermittent fevers, appears in his account of the ſucceſſion of the three ſtages of fever in paragraph DCLXVI, in the following words.

" Hence in a gentle degree of the diſeaſe, as cold is the moſt hurtful power, the conſequence is, that its effect is gradually taken off by the agreeable heat of the bed or the ſun, and the ſtrength thereby gradually brought forth. The heart and arteries gradually excited by the ſame heat, acquire vigor, and at laſt excited in their perſpiratory terminations by the ſame ſtimulus, the moſt hurtful ſymp-

tom

tom being thereby removed, they reſtore the hot fit, and afterwards carry on the ſame proceſs to the breaking out of ſweat."

To paſs over the logic of the firſt ſentence, and come to the merits of the ſubject—Can there be any thing more futile, abſurd and groundleſs? Is it not known to every one who ever experienced a paroxiſm of the intermittent fever, that neither the heat of the ſun nor the greateſt covering of clothes will abate the ſenſation of cold. Does he not know that a patient without any covering except his common apparel, may retire into a room where the direct rays of the ſun never enter, and the hot fit will ſucceed the cold almoſt as ſoon (if not quite) as if he was loaded with clothes and expoſed himſelf to the direct rays of the ſun. It is the power contained in the animal œconomy of generating heat that brings on the hot ſtage, or in other words, the *vis medicatrix*.

Mr. Sterne, in his Triſtam Shandy, ſays ſome people riſe by hanging great weights to ſmall wires: an obſervation applicable to this paragraph and the whole ſyſtem of Doctor Brown. If heat was ſuch a powerful mean in increaſing excitement, how would people ſuſtain the intenſe heat of forges, glaſs houſes and many other mechanic employments in which fire is the principal agent? There is a certain degree of heat neceſſary to create agreeable ſenſations, but whenever it is conſiderably raiſed above that point it ceaſes to

be

be ſtimulant. Nothing except an inſatiable unwarrantable deſire to promulgate a new theory could have ſo blinded this author as to endeavor to palm ſuch monſtrous abſurdities upon the public.

Doctor Cullen, in Aphoriſm XXXIV, ſays, as the hot ſtage of fevers is ſo conſtantly preceded by a cold ſtage, we preſume that the latter is the cauſe of the former. And as the cold ſtage conſiſts in a ſpaſmodic conſtriction of the extreme veſſels, thereby proving an indirect ſtimulus to the ſanguiferous ſyſtem, a reaction takes place, the ſpaſm is overcome, the hot and ſweating ſtages enſue and a complete ſolution of the paroxiſm follows. I for my part have no doubt of this being the mode in which nature attempts to relieve herſelf of any hurtful powers raiſed againſt the body; as debility ſimply will not, does not excite the languid action of the heart and arteries; if this was not the caſe, what would hinder the debility to increaſe ſo in the perſpiratory terminations of the arteries (as he fancies to call it) as at length to extinguiſh the vital principle in them entirely? The hurtful powers are conſtantly applied when the phænomena of the cold, hot and ſweating ſtages appear, and if providence had not wiſely implanted the *vis medicatrix* in our conſtitution, we ſhould fall an eaſy prey to the noxious agents that ſurround us. I have an equal degree of infidelity in the exhibition of ſtimulants in the hot ſtage, as I have in his theory. It is cuſtomary in the hot ſtage of

fevers

fevers to exhibit a dose of opium, but that it would be equally proper to exhibit a quantity of spirits possessing an equal degree of stimulant power (for we will admit the stimulant power of opium a moment) I deny. Opium is given in this stage on the foundation of its being an antispasmodic. Experience has proven it to have this effect. It promotes the secretions and thereby hastens the solution of the paroxism, but no stimulant to my knowledge will so generally have this effect. Our author in a former part of his work has imputed to the operation of opium the power of increasing the excitement on the surface, which may be brought forward to explain its good effects in this disorder. Admit it had a peculiarly exciting effect on the surface, it would not prove that it generally increased tone and inflammatory diathesis.

The operation of opium is no ways analagous to that of other diaphoretics, and as no stimulant possesses a like power of promoting perspiration, I therefore conclude, that the operation of opium in this instance (and most if not all others) is *sui generis*. It will by no means prove the admissibility of other stimulants in the hot stage of fevers. In fevers of great debility where there are no considerable exacerbatations, I admit we are to pay no regard to any stage, but to administer the most powerful stimuli. But when the paroxisms are clearly marked, I doubt and deny their admissibility. I have heretofore (I believe) asserted, that the cold and hot stages of intermittent fevers was an inflammatory condition

of the ſyſtem, and I preſume that ſtimulants would aggravate the paroxiſm. An increaſe of fever, a dryneſs of the ſkin, would be the infallible conſequence of the adminiſtration of powerful ſtimuli.

The power of the bark I can by no means eſteem ſo ſlightly as Doctor Brown—to ſay that it will turn out next to an impoſition, is impoſition itſelf. It is a medicine of ſovereign efficacy in all caſes of debility and putrefaction. To impute its good effects to the vehicle conjoined with it, is a great degree of ſtupidity and wilful blindneſs. When given in milk or treacle, I have never been able to obſerve any material difference in its efficacy. I can ſee no greater analogy between ſpirits, opium and common food, that between the peruvian bark and food. Neither opium or ſpirits are ever converted into juices. I am ſure of the two ſubſtances, opium and the cortex, that if either of them bears a greater analogy to the common ſupports of life than the other, that it is the latter. All barks poſſeſs a ſmall quantity of farinaceous matter—neither ſpirits nor opium poſſeſs any of it. The Otaheiten bread is the product of a tree; and I believe that any parts of vegetables would ſupport life better than opium or any chemical product. Barks I believe contain a ſmall quantity of acid, and I am not ſure but a little ſaccharine matter. Theſe remarks will be ſufficient I truſt, to refute his theory of fevers.

Paragraph DCLXXVIII. *The fluids by ſtagnating under the heat of the body, degenerate into that*

that foreign quality in a more extenſive ſenſe, called corruption, but in a more uncertain one, putrefaction.

It is to be remarked that the heat of the maſs of blood ariſes from friction and violent motion. From whence then is this ſuperlative degree of heat in putrefaction? The debility of the veſſels would prevent the generation of any extraordinary degree of heat. Indeed we would conclude a priori that a diminution of heat would be the certain conſequence of the ſtagnation of fluids. Heat being a powerful ſtimulant, we would ſuppoſe it would counteract the debility of the ſolids. That the fluids even in a ſtate of putrefaction, would contain a ſort of *vis medicatrix*. Heat is one of the principal remedies of our author, in ſthenic diſeaſe, and if the inherent heat, or the heat induced by the diſorder be active on the eſtabliſhed principles of this ſyſtem, although it be the conſequence of putrefaction, it would remove debility except its degree was ſo great as to induce indirect debility. If heat is a powerful agent in preſerving the healthy ſtate of excitement, it would be very eaſy to diminiſh occaſional redundancies of that element. The ſtimulant power of heat would ſeldom exceed the exigencies of ſtimulant power in diſeaſes of great debility. In a note on this paragraph, he ſays we are apt to refer every deviation of the fluids from their natural ſtate to one of the three fermentations. I have never to my recollection, in any author, obſerved any ſtate of the fluids compared

pared to the vinous or acetous fermentations. The changes that take place, in fluids circulating in living veſſels are imperceptible, we are entirely ignorant of them, and therefore we can compare them to no changes in inanimate matter. The putrefaction in animals and inanimate matter both by ſmell and appearance, reſemble one another very nearly.

The putrefaction of the circulating fluids is to be imputed to a diminution of vigor in the veſſels whereby they are left to their own inteſtine commotion and are in a condition pretty ſimilar to any other inanimate matter. But as long as the veſſels retain their accuſtomed vigor, the changes (if any take place) are not diſcoverable, and I am confident that the inherent vigor of the veſſels will always prevent either a vinous or acetous fermentation. The blood in every conſiderable deviation of the ſolids from their healthy tone and vigor, is followed with a correſpondent change. The component parts may exiſt in different proportions, but their preciſe and definite qualities are not diſcoverable. Heat has been ſuppoſed the offſpring of putrefaction. But is not putrefaction rather the offſpring of heat? There is a degree of heat generated in putrefying ſubſtances, but it is not excited in the ſame manner, and is not equal in degree to the healthy temperature of the body. The heat (if any) in a ſtate of putrefaction or mortification, is a morbid caſe of heat, and very ſeldom exiſts in the body. That the circulating

blood

blood is a perfectly mild bland fluid, I will not aſſert, but to infer the contrary, becauſe the urine, bile, and other ſecreted fluids are acrid, is a falſe illogical concluſion. The urine does not formally exiſt in the maſs of blood, and its acrid taſte and offenſive ſmell are undoubtedly owing to a change taking place in the ſecretory organ. What are thoſe diffuſible ſtimuli whoſe operation it is often neceſſary to moderate. I have no doubt we may adminiſter too large a quantity of ſtimulus. They then have a directly oppoſite effect to that intended. I have known ſtimulants and tonics act as cathartics. But in diſeaſes of great debility we very often fail with the moſt powerful ſtimulants, when the excitability is not worn out neither by age nor the exceſſive uſe of ſtimuli. If diſeaſe conſiſted in debility only, and we poſſeſſed ſtimulants whoſe operation was neceſſary to be checked, that long liſt or catalogue of diſorders which are now the *opprobrium medicorum* would no longer have an exiſtence. A few ſentences ſucceeding we have a formal account of the progreſſive manner in which excitement (when loſt) is reſtored through all the intricate windings of the digeſtive organs until it reaches the perſpiratory terminations of the arteries, the *primum mobile* of his ſyſtem.

The communication of excitement like the electric fluid is inſtantaneous. The very general diſtribution of nerves, and the conſent of the ſtomach with every other part, is ſuch that any impreſſion made on it is as readily communicated

to

to the moſt diſtant part as one ſituate more near it. This a mere parade of anatomical learning —It has no foundation in truth. The propriety of his five diſtinctions of local diſorders I doubt— The ſecond claſs in which he comprehends an inflammation of the ſtomach and inteſtines, appears devoid of foundation. I ſee no reaſon why an inflammation of the ſtomach and inteſtines has not as good a title to be denominated a general affection as an inflammation in the liver, ſpleen, tonſils, Trachea arteria, &c. The cauſes to which he imputes an inflammation of the ſtomach are more likely to exert their pernicious operation on the tonſils and œſophagus than on the ſtomach and inteſtines.

It is very ſeldom that ground glaſs gets into the ſtomach of any animal except rats. Cayan pepper too is eaten by many people without the leaſt injury, and in caſes where fiſh-bones are ſwallowed, I never heard of an inflammation of the ſtomach being the conſequence. The ſtomach has been known to diſſolve the moſt ſolid bones, and as long as the ſtomach retains its digeſtive power an inflammation is not to be expected. Many inſtances of inflammation of the ſtomach occur where it can be traced to no mechanical injury, and as they are preceded by all the ſymptoms characteriſtic of general inflammation, we are juſtified in aſſerting theſe affections to be general. The author muſt explain the different organization of the ſtomach and inteſtines from the ſhut viſcera before we concede to him

that

that they are not equally liable to general inflammation as the other viscera.

The fourth division of local disease is "*when contagion has been applied and generally diffused without affecting the excitement.*"

If as the author contends contagions possess a stimulant power, in what condition of the system do they not affect the excitement? It must be where a total torpor and inactivity of the system exists, or the contagion must be rendered inert, which is destroying the peculiar property of contagion. The venereal virus often occasions bubboes and hernia humoralis, but these complaints when entirely local, do not come within the limits of his fourth division. For in a recent gonorrhœa the contagion is locally applied; we have no evidence of its general diffusion over the system. These complaints have been observed to attend a confirmed lues, and in this case they have no claim to local affections, for the virus affects the excitement often in a most serious and alarming manner. Swellings in the groins in pestilential fever or plague, although they are the sequel of a general disorder, cannot with propriety be said to be entirely local. Their cause being a contagious diathesis and a general affection will (as they depend on the diathesis) exclude them from local diseases. Independent of these objections to his division of local disorders, it would not be departing from the truth to affirm, that all disorders depend upon the diathesis

—even

—even boils moſt commonly occur in plethoric ſanguinary habits. It is the living principle which determines a greater quantity of blood to the part in boils and conſequently inflammation —Further, we almoſt always employ general remedies in local diſorders, and often with better effect than topical applications. Few complaints are of ſo trivial and unimportant a nature as not in ſome ſhape to affect the excitement.

The fifth part or diviſion of local diſeaſes, "*is where poiſons enter the body and circulate through the ſyſtem without effecting the excitement, except by producing ſome leſion or local injury, and in conſequence of that injury diſturbance over the ſyſtem.*"

Vegetable poiſons when applied to the ſurface of the body, often occaſion fever & other ſymptoms of their operation on the excitement. Indeed it is impoſſible that poiſons can be applied to ſo large a portion of the body as the ſkin, without affecting the principle of life or ſenſibility of the ſyſtem. The mode of operation of vegetable poiſon applied externally is not fully underſtood or explained. It may be by abſorption of the effluvia that are emitted from vegetable poiſons, or by a powerful ſtimulant operation. Whether contact is neceſſary to give effect to vegetable poiſon, I am uncertain; but I think that I have known perſons affected without the recollection of any contact. In whatever manner they

they operate, I cannot conceive of a poiſon being generally diffuſed through the veſſels without affecting the excitement. The cutaneous inflammation that originates from vegetable poiſons, is (as Doctor Brown ſays of the univerſal cake of inflamed puſtules in the ſmall pox) ſuch an exquiſite ſtimulus, that the affection of the excitement muſt be the inevitable conſequence. Even if the particles enter not the ſanguiferous ſyſtem, the diſorder continues local but a very ſhort time: And if they do not enter the ſyſtem their effect, the diſorder, does not come within the meaning or definition of this diviſion of local diſorders. The bites of poiſonous animals cannot with any propriety be called a local diſorder. The bite of an animal not poiſonous is a local diſorder. It is a mere ſolution of continuity and no otherwiſe affects the excitement than a ſolution of continuity made by an inſtrument, except the lips of the wound are generally more ragged, mangled and difficult to heal. But the poiſon of animals ſoon diffuſes itſelf over the ſyſtem, the excitement is raiſed to a violent heighth and death is often the unhappy conſequence. It would puzzle the author or any of his adherents to advance many inſtances of poiſons being generally diffuſed through all the veſſels without producing an alteration of excitement. I do not recollect one ſolitary inſtance.

To conclude our remarks on this work, we will ſubjoin a few general obſervations.

Let us pauſe a moment—Let us concentre all

the rays of medical light that emanate from this syſtem, into a focus. Can we ſay in ſincerity that the author has any pretenſions to originality of principle? Can we ſay, that the nicknamed Brunonian ſyſtem is the ſyſtem of nature? Can we ſay that he has uſhered in the millineum of phyſic? The principle of excitement and excitability, are abſtract, arbitrary principles. They are plainly derived from Cullen's theory of the mania. The idea was at leaſt taken from Cullen's ſyſtem, and although they are words which are as applicable as any language can afford to convey an idea of the ſtate of the mind or body when we don't mean to point out their mechanical conditions, yet they convey no ſcientific idea of the ſtate of the body in health and diſeaſe. The viſionary hypotheſis of Staahl, the ſuppoſed Centor of Boerhaave, the gas of Van Helmont, or the vital fire of Shebbeare, would either of them have ſuited his purpoſe as well. All the difference is, that the word excitement expreſſes the effect of the living principle, and former theoriſts have attempted to explain the nature, ſeat and modus operandi of the living principle. The term excitement is uſed by him as ſynonymous with the living ſtate, and every one the leaſt acquainted with language knows, that barely ſaying that the living ſtate is in proper degree or otherwiſe, is ſaying nothing at all.—What practical utility is there in the boaſted ſcale of the author. The vital principle (if a fluid) is of ſuch a nature that a guaging rod cannot be immerſed to point out its riſe and fall. And although

though it may be affected by the denſity or tenuity of the circumambient air, the ſcale of our author can in no caſe point out the viciſſitudes it undergoes. His ſyſtem ſtands entirely detached and unconnected with anatomy, phyſiology and every branch of medicine. Does his language convey a philoſophical idea of the ſtates of health and diſeaſe?

Inquire what occaſions the different ſecretions of the body, how muſcular contraction is performed. Excitement is the anſwer. After this pertinent anſwer, what do we know more than before? Look through this boaſted ſyſtem. Are not the features of Cullen's ſyſtem perceptible? Take notice of the excitement being higher on the ſurface than any other part. Take notice of medicine operating on the extreme veſſels, and of the energy of the brain being diminiſhed and reſtored. From what ſource are thoſe ideas derived? In every other ſyſtem, the author's have ſought for the vital principle through the channel of anatomy. The knowledge of anatomy has always been conſidered important to lay the foundation of phyſiology. And as phyſiology is a branch of medicine that embraces the knowledge of the living principle, and explains the different condition of the ſimple ſolids both as connected with and detached from the living principle, an intimate connection exiſts between the two branches. But our author takes a ſhorter cut to ſcience. He has invented a method to obtain a knowledge of language without

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a previous acquaintance with the alphabet. Has the author improved nosology by discarding all former classification of disorders, and substituting his two forms, the sthenia and asthenia. Is he entitled to a very abundant score of merit for the coinage and application of these words to the science of medicine? Has he not multiplied instead of diminishing disorders by the introduction of his two forms and subdividing them into the mild and violent? Former nosologists may have made arbitrary arrangements; they may have made supernumerary distinctions. But, is the new nosology superior to the old? Is not the old nosology better calculated for the instruction of pupils than the new? I am always disposed to give every man that portion of merit to which he is entitled; but, after a thorough survey of the work before us, I cannot discover any thing new or useful, except the distinction of direct and indirect debility. And even this distinction has been mentioned by Dickinson, so that it is doubtful whether Doctor Brown was the author of it. Whoever first started the idea and the variety of practice founded on these distinctions, is entitled to some share of credit. The idea has been known and observed long before the publication of the Elementa Medicinæ. Doctor Ruth, on the effects of spiritous liquors observes, that to break the habit of drunkenness, it is necessary to gradually diminish the quantity, or substitute some other stimulus. The idea was known and discovered before the invention of the term.

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The principles I have eſtabliſhed or embibed will not allow me to go far in his mode of cure. But whenever I have attempted, I have uniformly failed. The ſucceſs of an oppoſite plan of cure adopted by Doctor Ruſh in the yellow fever does not coincide and harmonize with his principles. The exhibition of Calomel and Jalap, cannot be juſtifiable on the principles of this ſyſtem. Suppoſe the contagion induced indirect debility, the gradual diminution of ſtimuli would be the proper mode of cure. The variety of ſymptoms that occur in diſorders, cannot be imputed to debility merely. I cannot aſſent to the identity and ſameneſs of all the aſthenic form of diſeaſes. They differ not in degree of debility only. Their cauſes are often ſpecifically different, their ſymptoms are as various and oppoſite as the ſymptoms of the other form. I will not deny that the mode of operation of all ſubſtances on the body reſemble one another, and that they in ſome meaſure ſtimulate. But the queſtion is whether their ſtimulant power is ſufficient to maintain health? If they do not, they may with propriety be called ſedative powers.

The extenſion of the ſtimulant plan of cure to ſo many diſorders as the author has done, I do not think diſcovers a great acquaintance with the effect and operation of medicine. It was carried full far enough before the publication of the Elementa Medicinæ. Many diſorders of apparent debility have given way to what he calls the debilitating plan, after the ſtimulant plan had been exhauſted.

exhauſted. The author, in my opinion fell a victim to his own theory. It has been productive of miſchievous conſequences in this country if I may rely on the judgment of others. Syſtems have in many inſtances had a baneful influence in the adminiſtration of medicine. This ſyſtem has (I believe) ſeduced not only the young and inexperienced, but the old and wary. It is a falſe light. The author was not contented with barely delivering his opinion, but he muſt miſrepreſent the ſcience of medicine prior to his diſcovery. I would aſk any rational phyſician whether the ſum total of medical power is enlarged by the diſcovery. Is not the ſyſtem of Brown a corruption of Cullen's ſyſtem? I pledge myſelf to the world, that had not Cullen's ſyſtem made its appearance, that the ſyſtem of Brown would never have ſeen the light. With all his art and labour to diverſify it, the ſource from which it is drawn is obvious. The circumſtances under which it was compoſed were ſuſpicious. The difference between Cullen's and Boerhaave's ſyſtem is, that the former diſcarded the humoral pathology and accounted for the moſt of diſorders upon changes taking place in the ſolids. Is not Brown's ſyſtem built on the ſame foundation? No. He directs his medicines to the excitement. But no change can be effected in the excitement, except the medicines alter the ſtate of the ſolids. What idea does the author mean to convey by deficient excitement—excitement in proper degree or increaſed excitement? They convey ſuperficial ideas of the ſtate of the animal

œconomy,

œconomy, and however fond the author may be of the novelty of expreſſion, if they convey any other idea than debility or atony, and inflammatory diatheſis of Doctor Cullen, I muſt own I cannot diſcover it.

As the baſis of his pathology is the ſame as Cullen's, what merit can he claim, except the coinage of a few words of Greek derivation.—Inſtead of publiſhing a new ſyſtem he has forcibly entered upon the premiſes of Cullen, endeavored to demoliſh the beautiful edifice the latter had reared, and like an ignorant architect, erected it in a new and bungling manner, whereby its beauty, ſimplicity, and utility are entirely deſtroyed. The ſtyle has no claim to elegance; in many places it is very deficient in perſpicuity, and in no part of it is his ſentences conſtructed in a neat, elegant manner. This would be a ſmall fault, did it contain principles calculated and ſuitable to govern the adminiſtration of medicine.

We live in an enlightened age, and every principle ſhould be brought to the teſt of experience. The ſyſtem of Brown, would have appeared very well when men wrote ſyſtems in their cloſets. But in the preſent day, when experiment has become faſhionable in the ſciences, the truth of his principles will not be admitted except they agree with experience.

Fancy has made wild excurſions, invention has been ſtretched in ſearch of an unerring principle to govern the adminiſtration of medicine—but all has

has been comparatively fruitleſs. Experience is the only infallible teſt of the operation of medicines. Theories may ſerve to guide and govern in general caſes; but wherever a theoretical principle is held up as an unerring guide, we are ſure to be deceived in its application. In no theory is it more conſpicuous than the one under diſcuſſion; it attempts to make the application of his general principle too univerſal. Experience demonſtrates its futility, and reaſon diſcovers that the principle is viſionary and Utopian.

The term excitability can convey no other meaning than that the animal œconomy is capable of being excited and animated by certain external agents. And the term excitement, conveys no other meaning than that the body is under the operation of exciting powers. The philoſophical mind will eaſily diſcern how far theſe terms go to explain the different ſtates of health and diſeaſe. Had the author ſubſtituted heat, inſtead of excitement, it would have ſerved him full as well to explain the pathology of diſorders. For as the accumulation of excitability (if it is proper to aſcribe it to the negation or abſence of a poſitive principle) is nothing more or leſs than an abſence of excitement, and as cold is nothing more or leſs than a privation of heat, I think the term heat and cold would have been full as apt to explain the healthy and morbid ſtates of the animal œconomy as the terms uſed in the Elements of Medicine. Heat is a more familiar phraſe than excitement, and more obvious to common capa-

city.

city. And as they both expreſs the *effect* only of the living principle, the former appears to me preferable. What adds weight to this opinion is, that as heat in moſt caſes appears to be in pretty exact proportion to the vigor of life, and cold on the other hand reſembles a ſtate of death—it appears moſt natural to give theſe terms place to the new coined phraſes of Doctor Brown.

The ſcale of our author is inconſiſtent with itſelf. For agreeably to the true principles of this ſyſtem whenever indirect debility commences, the excitement muſt fall, and as the excitement falls it muſt repaſs through the grade of health inſtead of riſing to 80° the point of death.

The co-exiſtence of indirect debility and increaſed excitement are incompatible. The excitement never can riſe up to 80°. For the human body is capable of a certain degree of activity of muſcular fibre, and as death never takes place until all exertion and activity are nearly or quite exhauſted, it therefore clearly follows, that previous to death the excitement muſt be diminiſhed in room of increaſing to 80° his imaginary point of death. The condition of muſcular fibre when the excitement has riſen to 70° or fallen to 10° I acknowledge to be entirely different, and require a different treatment to reſtore the healthy ſtate. But debility direct or indirect always implies want of excitement. Our author has arranged the gout in his ſcale as a diſorder pro-

ceeding from directly debilitating powers, and in the paragraphs of which he treats of its nature and mode of cure, he affirms it to be the offspring of indirectly debilitating powers, and prescribes a mode of cure adapted to direct debility. An inconsistence of such consequence ought not to pass unnoticed. If we may credit the reports of others, the gout often makes its appearance in persons who live luxuriously without the addition of directly debilitating powers. Persons of this description are not apt to abate their mode of living either in quantity or quality. On the other hand it is asserted by men of experience, that the appetite of podagrics are the most vigorous the day preceding the attack. And it is often with the utmost difficulty that physicians can prevail upon podagric patients to observe that degree of abstinence necessary to restore them to health. An attempt to discriminate disorders as they originate from direct or indirect debility, is equally, if not more frivolous than nosological arrangements. The same disorder may proceed from want or an excess of exciting power. I acknowledge, could we certainly discover the quantity of operation of both sets of powers, it might serve some purpose in the administration of medicine. But they are often so combined, that it would be with the utmost difficulty to ascertain the amount and difference of their operation. This is evident in the author's arrangement of the gout; for he differs from himself and all former physicians in ascribing the gout to low living, and in affirming that although indirectly debilitating

powers

powers may have been applied a paroxism of the gout never comes on without the application of directly debilitating powers.—And I will further observe, that the phrase *directly debilitating powers* is synonimous with sedative powers. It manifests a great degree of infatuation for a person to combat the chimeras of his own imagination.

The method of cure enjoined in this work appears to me a continual warfare with experience. The disorders of the climate of America, Scotland and the north of Europe, often partake of an inflammatory nature. They very seldom give way without premising some considerable evacuations prior to the administration of stimulants. The necessity of evacuations has been denied by many physicians, and particularly by Doctor Saunders, in his pamphlet on the efficacy of the cortex. But I have in many instances administered the cortex and wine without any considerable relief until evacuations were made by vomit, stool and perspiration. In remittent fevers of this climate, when they had baffled the power of stimulants, and when great marks of debility existed, I have seen manifest advantage by the administration of Calomel so as to produce two or three stools. In an obstruction of the Catamenia of a long standing, when paleness, want of appetite and a feeble pulse, all manifested great debility, I have observed considerable advantages from Cathartics and the application of Epispastics prior to the administration of tonics and stimu-

lants,

lants. The good effects of the Epiſpaſtics I imputed to the evacuation. If not only the diſorders of Northern climates, but the moſt putrid diſorders of tropical regions are cured by evacuations, what ſhall we think of the boaſted efficacy of ſtimulants. Veneſection has lately been pretty generally adopted in the Weſt-Indies in the yellow fever, and as we are informed with conſiderable ſucceſs.

The inexperience of youth and the dogmatiſm of old age, have greatly impeded the improvement of the healing art. The character of Doctor Brown, ſeems to be a combination of both. When we depart from the field of experience and adventure into the regions of conjecture, we are liable to deception from every quarter. Pleaſed with the fancies of our creation, we hug them with unſpeakable delight. They bar the mind againſt the reception of new ideas; and the mind deſigned for increaſing perfectibility continues ſtationary.

The theory of Doctor Brown, can certainly in point of utility claim no pre-eminence over that of Doctor Cullen. And although the latter is beſet with many difficulties, the former is open to more ſolid objections. A ſtill further objection to the principles laid down by Doctor Brown, bolts in upon my mind. People who take a degree of exerciſe, ſleep and food, ſufficient to keep the excitement at 40°, the healthy ſtate do ſtill, from the operation of marſh effluvia and

contagious

contagious matter fall into fevers. Could this happen if they were of a ſtimulating nature. A circumſtance which renders it improbable is, that children and adults of weakly conſtitutions are more liable to fevers than perſons of a robuſt and vigorous habit. If they induced fevers by an indirectly debilitating operation, the robuſt and the vigorous would be the moſt ſubject to theſe affections—For when the excitement ſtands at 40° or higher, a leſs degree of ſtimulant power would produce a diſorder than when it ſtood at 30° or lower. The highly excited condition of the body, and the highly exciting power of contagion would ſoon throw the ſyſtem into diſorder. But experience evinces that the vigorous and healthy eſcape the effect of theſe noxious agents oftener than the weak and valetudinary. As all ſubſtances in nature agreeably to this theory have a homogeneous operation on the animal œconomy, provided we could concenter and apportion them to the excitability, why may not marſh effluvia and contagion nouriſh and ſupport life in the ſame manner as vegetable and animal food. This, could it be reduced to practice, would be a moſt capital diſcovery and of the laſt importance to the ſuſtenance of man. I preſume however, that phyſicians have not ſo far given up their judgement as to have very ſanguine expectations of realiſing this chimerical notion. If we appeal to an unerring guide, *experience*, we will be ſatisfied that in all conditions of the vital principle marſh effluvia and contagion exert a very baneful operation on the living ſyſ-

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tem, that under no modification are they suited to restore health, but on the contrary, peculiarly calculated to generate disease. Were they as our author asserts, of a stimulating nature, they would be equally as proper as any equal stimulant power in persons labouring under direct debility. A still further difficulty occurs in reconciling Doctor Brown's general principles with facts, is that the same quantity of ardent spirits taken on an empty stomach will induce ebriety much sooner than when the stomach is loaded with food. Whereas if intoxication was the effect of the indirectly debilitating operation of the liquor a full stomach as it must coincide by its stimulating power with the exciting or stimulating operation of ardent spirits would more certainly and sooner take place with an equal quantity of spirits on a full than an empty stomach. I believe it may be laid down as a pretty general rule, that all substances which offend the senses are of a sedative nature. We observe too, that aromatics as they possess an agreeable odor are of a stimulating nature. The former observation is strengthened by the well known fact that odors in some people induce *syncopes*. And although some odors that are pleasant to the most of people are offensive to others, yet as by an idiosyncracy of constitution, they prove injurious, it by no means impairs the force of the observation that odors which are offensive to the senses are of a sedative nature.

A further proof of this observation is, that the

senses

ſenſes ſeem deſigned to diſcriminate between ſubſtances, which are deleterious, and thoſe that are ſalutary. For we find that ſubſtances which offend both ſmell and taſte generally give uneaſineſs when taken into the ſtomach. Animals too are dictated by the ſame criterion in the choice of food.

I have, in the courſe of theſe obſervations conceded a ſtate of excitement above the healthy point to be ſynonymous with phlogiſtic diatheſis. But I will remark a very ſtriking difference in the treatment of a genuine phlogiſtic diatheſis and a ſtate of excitement ſupervening the uſe of diffuſible ſtimuli in a diſeaſe of debility. In the former venæſection is always of manifeſt advantage, in the latter I conceive it would be very injurious. A forbearance in the uſe of ſtimuli in the latter, would be the moſt judicious procedure. A true phlogiſtic diatheſis never can exiſt unleſs the digeſtive and aſſimilating organs have been vigorous, and a large quantity of nutriment extracted and applied to the fibres of the body. It is true that when any conſiderable injury has been inflicted in any part of the body, a phlogiſtic diatheſis often occurs; but rarely (if ever) except the perſon enjoyed good health. The impreſſion of ſtimuli on the ſtomach is tranſitory. And although by their frequent repetition we often invigorate the ſtomach to overcome the food, yet a complete reſtoration of health is not to be expected until a vigorous appetite and digeſtion has for ſome time exiſted.

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The theory of Doctor Brown does not correſpond with my experience. It is my opinion, that could the grave yards give an account why they are ſo populous, not a ſmall ſhare would be imputed to the new demonſtrated ſyſtem of medicine. The remarkable diſcovery that a certain degree of animation or excitement is neceſſary to the healthy ſtate of every living ſubſtance, does not characteriſe this ſyſtem the ſyſtem of nature. The truth of that opinion no one will controvert, but the practical utility of the diſcovery is queſtionable. What light does it reflect on the growth and nouriſhment of vegetables to ſay that their healthy ſtate depends on a certain arbitrary number of degrees of excitement. The toil and labour of our author reminds me of that paſſage of Horace—*Montes parturiunt naſcetur ridiculus mus.*

The aſſertion of the author, reſpecting the ſudden efficacy of ſtimulants in diſorders which have heretofore been eſteemed incurable, appear to me poſitive falſehoods. A ſyſtem engendered through hatred and ſpite, and ſupported by improbable truths ſhould not meet with a very favorable acceptance with the candid and judicious. I always ſuſpect the truth of a man's aſſertion when he uſes violence to impreſs belief. A ſimple relation of facts is ſufficient for the modeſt and unaſſuming.

To conclude our remarks upon this work—I hope and truſt that my brethren of the Faculty will not be ſeduced by this *ignuus fatuus* into the wild, devious paths of experiment and medical enthuſiaſm.

ERRATA.

Page.	Line.	
4	— 20	for diviriſified, read diverſified.
6	— 21	for diſorder—diſorders.
13	— 19	for was—be.
15	— 11	for reſiſts—reſiſt.
19	— 1	for operation—effect.
20	— 26	for untagible—untangible.
22	— 14	for deliterious—deleterious.
30	— 13	omit the word *that*.
31	— 16	inquiry—enquiry.
33	— 21 & 26	variolus—variolous.
36	— 4	dody—body.
37	— 22	deſcompoſing—decompoſing.
51	— 5	overcome—overcame.
53	— 20	elementary—alimentary.
58	— 11	centrated—concentrated.
60	— 24	inquiry—enquiry.
61	— 30	for—inſtead of procraſtinating, read—rather than procraſtinate.
65	— 23	omit the words, *that is*.
66	— 23	increaſes—increaſe.
68	— 22	poſſeſſes—poſſeſs.
70	— 9	pleuretic—pleuritic.
84	— 23 & 27	omit the word *other*.
90	— 24	an—in.

William [illegible]
Is my name